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(Plates I.–IV.)

[Read January 22, 1902.]

The first and second parts of our researches on the above subject appeared three years ago in the Transactions of the Society,* having been read in February and May, 1899. The present part deals with

(a) alloys of iron which we have found to be non-magnetic, or nearly non-magnetic, even in strong magnetic fields;

(b) alloys of iron which within a certain range of magnetic field we have found to be more magnetic than the purest iron obtainable in commerce.

It is obvious that these two classes of iron alloys or steels are of permanent interest and importance both from a theoretical as well as from a practical point of view.

In our previous paper a full description was given of the process of manufacture and the method of testing these alloys. Here we will only state that the alloys were all made and analysed at the Hecla Steel Works, Sheffield, of which one of the authors is Managing Director, and the magnetic and electric measurements were all made in the Physical Laboratory of the Royal College of Science, Dublin. The specimens, many of which are entirely new metallurgical products, were rolled into the form of cylindrical rods 102 cms. long and about 0·5 cm. diameter (No. 5 B. W. G.); they were then very slowly and carefully annealed from a white heat. Duplicates of a few specimens were made which were subjected to different

* See Vol. vii., Part iv., p. 67.

heat treatment, and were specially marked. The method of magnetic measurement adopted was the "magnetometric method," the rods being placed in a magnetising solenoid as described in our former paper. This is the most rapid and convenient form of measurement, and for comparative purposes does very well, but it is not suited for exact absolute measurements. Hence in the case of a few highly permeable specimens the ballistic method was adopted, the specimens being made into the form of rings which were carefully annealed and wound with primary and secondary coils. Another set of experiments were also made with these highly permeable alloys forged and turned into the form of short cylindrical rods, so that they could be tested by the "yoke method" in Ewing's permeability bridge. The results of these comparative tests will be given in this paper. A still further comparison was made by turning some of the original long metre rods down to a much smaller diameter, keeping their length the same, and after annealing, again testing them by the magnetometric method. These precautions were only necessary in specimens of very high magnetic permeability; with the non-magnetic or nearly non-magnetic alloys, with which we will deal first, one method is nearly as good as another, provided a uniform magnetic field can be obtained. These latter specimens were therefore in the form of the original rods 102 cm. long and $\frac{1}{2}$ cm. diameter.

(A)—Iron Alloys nearly or wholly Non-Magnetic.

In previous papers we have shown that the addition of certain elements to iron rendered the alloy practically non-magnetic, and we gave a list of seven such alloys, reserving to a subsequent paper the fuller examination of this part of our subject.* This we now bring before the Society.

In the accompanying Table I. is given the percentage composition of a series of iron alloys or steels which, even when carefully annealed, are non-magnetic in a field which nearly saturates pure iron. Employing a delicate reflecting magnetometer, and increasing the strength of the magnetic field seven-fold, viz. to 320 C. G. S. units, a slight magnetisation was observed in some of the specimens; the minute magnetic susceptibility thus revealed was measured, and the result is given in the table. The first column gives the mark attached to each specimen at the Hecla works; then follows the result of the chemical analysis, the iron being estimated by difference; then the intensity of magnetisation, I, in a field of 320 C. G. S.—the best soft iron has an intensity of about 1600 in this field.

It must be remembered that all the specimens in Table I. were very carefully *annealed*, a few duplicate specimens were left "as rolled," not annealed nor specially hardened; these are marked "Un."

* Trans. R.D.S., vol. vii., p. 120.

TABLE I.

Non-Magnetic and nearly Non-Magnetic Steels.

Magnetising Force 320 C.G.S.

Mark.	Chemical Composition.	Intensity of Magnetisation in a field of 320.
1388	Fe, 86·74; C, 0·26; Mn, 13·0, ...	65
1010	Fe, 85·77; C, 1·23; Mn, 13·0, ...	123
,, Un.	,, ,, ,, ...	0
1379 D	Fe, 84·64; C, 0·15; Mn, 15·2, ...	0
30	Fe, 83·25; C, 1·5; Mn, 15·25, ...	82
598	Fe, 80·96; C, 1·54; Mn, 18·5, ...	49
1424	Fe, 76·58; C, 0·83; Mn, 5·9; Ni, 14·44; Cu, 2·25,	20
1109	Fe, 80·16; C, 0·8; Mn, 5·04; Ni, 14·55, . .	3
,, Un.	,, ,, ,, ,, . .	0
1414 A	Fe, 75·36; C, 0·6; Mn, 5·04; Ni, 19, . .	16
,, Un.	,, ,, ,, ,, . .	3
1414 B	Fe, 69·36; C, 0·6; Mn, 5·04; Ni, 25, . .	7
,, Un.	,, ,, ,, ,, . .	5
1339	Fe, 88·22; C, 1·21; Mn, 8; Ni, 2·57, .	7
1313	Fe, 79·35; C, 1·4; Mn, 10·25; Ni, 9, . .	49
1343 A	Fe, 84·71; C, 1·34; Mn, 11·1; W, 2·85, . .	30
,, B	Fe, 86·61; C, 1·08; Mn, 10·2; W, 2·11, . .	5
620	Fe, 78·12; C, 0·88; Mn, 17·5; Cr, 3·5, . .	46

The first thing that strikes one in this table is the remarkable effect produced by comparatively small quantities of another element alloyed with iron in destroying or altering the magnetic susceptibility of the latter: 13 per cent. of manganese added to iron destroys the magnetic character of the iron; the steel thus produced is, for all practical purposes, as inert to magnetism as zinc or stone. Still more striking is this effect in composite alloys of iron; nickel-steel is strongly magnetic, even an alloy of iron with 14, 19, or 25 per cent. of nickel is fairly magnetic, but add 5 per cent. of manganese to any of these high percentage nickel-steels and they become non-magnetic, albeit, a 5 per cent. manganese-steel is very fairly magnetic, as shown in our previous paper. Again, an 8 per cent. manganese-steel has a considerable intensity of magnetisation even in much lower fields than 320 units, but add $2\frac{1}{2}$ per cent. of nickel to this manganese-steel and it becomes non-magnetic; and yet in itself a $2\frac{1}{2}$ per cent. nickel-steel is almost as magnetic as

pure iron in low fields. And so with other metals alloyed with manganese-steel; a 10 or 11 per cent. manganese-steel is slightly magnetic, but entirely loses its magnetic properties when tungsten-steel, in itself magnetic, is alloyed with it. Curiously enough, the addition of the non-magnetic metal *copper*, added to an almost non-magnetic nickel-manganese-steel, slightly increases the magnetic susceptibility of the composite alloy, and the same seems true of chromium added to a high and practically non-magnetic manganese-steel. Both these results are probably due to the softening effect of the added metal, for copper alloyed with a hard steel softens it considerably.

In fact the slight differences in magnetic susceptibility observed in Table I. are probably entirely due to differences in hardness of the various alloys. Tested with a file, the order of hardness coincided with that of magnetic susceptibility, the hardest alloys being the least magnetic. Hence it is not surprising to find that an iron alloy which is fairly magnetic when annealed, is made nearly or wholly non-magnetic when hardened. This is especially noticeable in the 13 per cent. manganese-steel marked 1,010, as shown in Table I. The effect of annealing shows itself to a less extent in other specimens in Table I., but is more conspicuous in Table II. below: the magnetic field in this case being only 45.

TABLE II.

EFFECT OF ANNEALING.

Maker's Mark.	Principal Constituents.	Intensity of Magnetisation in a field of 45.
1447 A	Fe, 86·1; Ni, 12·7, ...	370 Ann.
,,	,, ,, ...	16 Un.
1447 B	Fe, 86·4; Ni, 12·1, ...	345 Ann.
,,	,, ,, ...	8 Un.
1450 A	Fe, 85·1; Ni, 12·2; Cr, 2·0, ...	295 Ann.
,,	,, ,, ...	16 Un.
1287 K	Fe, 79·0; Ni, 19·6, ...	644 Ann.
,,	,, ,, ...	473 Un.
1430 A*	Fe, 86·7; Cr, 8·9; Mn, 3·1, ...	1046 Ann.
,,	,, ,, ,, ...	37 Un.
1411	Fe, 77·3; Ni, 14·1; Mn, 5·3; Al, 2·3, . .	65 Ann.
,,	,, ,, ,, ,, . . .	100 Un.
1449	Fe, 67·1; Ni, 31·4, ...	195 Ann.
,,	,, ,, ...	346 Un.

* This specimen has 1·3 of carbon.

The last two of the above alloys 1411 and 1449 were found to have a lower intensity of magnetisation when annealed—*i.e.* when cooled slowly from whiteness—than when unannealed: but as was shown in our previous paper,* the effect of slow cooling on the alloy, marked 1411, is to render it harder to the file, and this again coincides with a worse magnetic condition: the behaviour of 1449 is referred to on the next page.

This difference in hardness probably explains the remarkable magnetic effect produced by a high percentage of carbon in manganese steels or in nickel-manganese-steels. In our previous paper we pointed out that an excess of carbon in high manganese-steels softened them and increased their magnetic susceptibility. This fact comes out very strikingly in the foregoing tables. With low carbon a 15, or even 13, per cent. manganese-steel is practically non-magnetic, in all ordinary magnetic fields, but high carbon-steels, having the same percentage of manganese, can be slightly magnetised. Even in an $18\frac{1}{2}$ per cent. manganese-steel the presence of $1\frac{1}{2}$ per cent. of carbon enables a feeble magnetisation to be imparted. The same thing is observed in the nickel-manganese-steels.

The importance of heat treatment, as affecting the magnetic character of steel, is well known, but it must be borne in mind (as stated above) that with certain steels, especially high manganese-steels, the effect of slow cooling from a white heat is the reverse of that produced in ordinary steel; it softens the latter but hardens the former. Sudden quenching of a carbon-steel renders it hard and brittle, but causes a high manganese-steel or nickel-manganese-steel to become tough and to some extent soft.

It is hardly necessary to point out the great commercial and practical importance of non-magnetic steels of considerable tensile strength, if their cost of production be not prohibitive, and if they can be machined without much difficulty. With regard to the latter point, the high manganese-steels which are non-magnetic, even when toughened by sudden quenching in water, are so intensely hard that they cannot be planed or turned, and hence can only be used rolled or in the form of castings. But several of the non-magnetic steels in Table I. are by no means so hard. These specimens can be planed or turned, and are excellent homogeneous alloys, both from a metallurgical and mechanical point of view. The least hard of these non-magnetic iron alloys was the one marked 1313, a nickel-manganese-steel; but others were also workable as judged by the file.

Though it may not at present be commercially profitable to construct non-magnetic ships of one of these alloys, the increase of safety in navigation would be great if this could be done. There are, however, many purposes in electrical engineering, as well as in the construction of clocks and watches, where non-magnetic

* Trans. Roy. Dub Soc., vol. vii., p. 97.

steel, of good mechanical properties like some of the above, would be invaluable, and where the question of the cost of material is comparatively unimportant.

The conversion of a strongly magnetic body into a non-magnetic one by alloying it with another magnetic body is a matter of great theoretic interest, and one that awaits full explanation. We have referred in the sequel to some considerations which bear upon this matter.

(B)—Alloys of Iron more Magnetic than Pure Iron.

We now pass to the opposite class of iron alloys, wherein we have found, as mentioned in our previous paper, that the addition of certain elements, such as nickel, silicon, and aluminium, *increases* the magnetic susceptibility in low fields, even of the purest and best iron commercially obtainable.

(1) The magnetic properties of the alloys of iron with *nickel* have been examined by the late Dr. Hopkinson, F.R.S., and others, but the specimens they used contained, as a rule, high carbon, and were really, what they were termed, nickel-steels. Hopkinson, as is well known, first showed that high nickel-steels could be made to exist at ordinary temperatures in a magnetic or non-magnetic state according to the previous heat treatment they had received. The specimens of nickel-iron alloys here examined had as a rule a low percentage of carbon and other impurities, and were carefully annealed. Rejecting those specimens which contained the largest percentage of carbon, the curve of magnetic permeability with increasing percentage of nickel is shown in Plate I. It will be noticed that in a field of 8 C.G.S. units the permeability rapidly falls after 4 per cent. of nickel is added, and the annealed alloy becomes nearly non-magnetic in low fields when 12 per cent. is reached, but after 25 per cent. a sudden increase of permeability occurs up to the highest percentage reached, when 31 per cent. of nickel is alloyed with iron. This curve of permeability closely resembles the curve of tensile strength and hardness of these alloys which we gave in our former paper.*

When the nickel does not exceed 3 per cent. the alloy is nearly as good magnetically as very soft iron, and when the magnetic field is very low, under one unit, the permeability of a 30 or 31 per cent. nickel-iron alloy exceeds the best and softest iron. So soft is this annealed high nickel-iron alloy (marked 1449) that it is magnetically saturated in a field of 4 units, in fact it is highly magnetised when the vertical force of the Earth's magnetism alone acts upon it, more so in this field than the purest and softest annealed iron. But, as will be seen from the

* Trans. Roy. Dub. Soc., vol. vii., p. 112.

accompanying H and B curves, fig. 1, only a low maximum induction can be imparted to this alloy even in the highest fields. The unannealed alloy being somewhat harder is much less permeable in low fields; but when the field exceeds

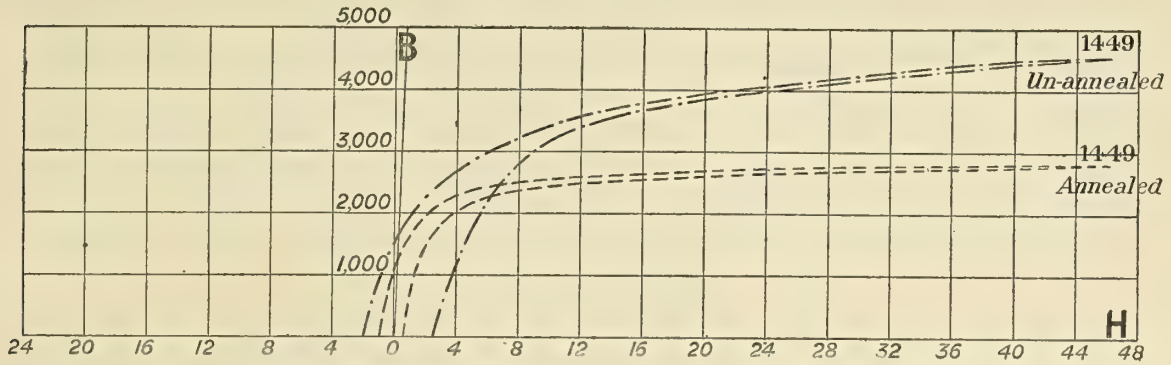


Fig. 1.—Upper part of cyclic curves of 31 per cent. nickel-iron alloy.

6 C. G. S. units, a greater intensity of magnetisation is produced in this unannealed alloy: hence the apparent anomaly shown by this specimen in Table II.

(2) We now come to the *silicon-iron* alloys. Here, as will be seen from Plate I., the permeability in a field of 8 is greater than iron if the silicon does not exceed 3 per cent., judging from the few specimens which we have at our disposal. In order to

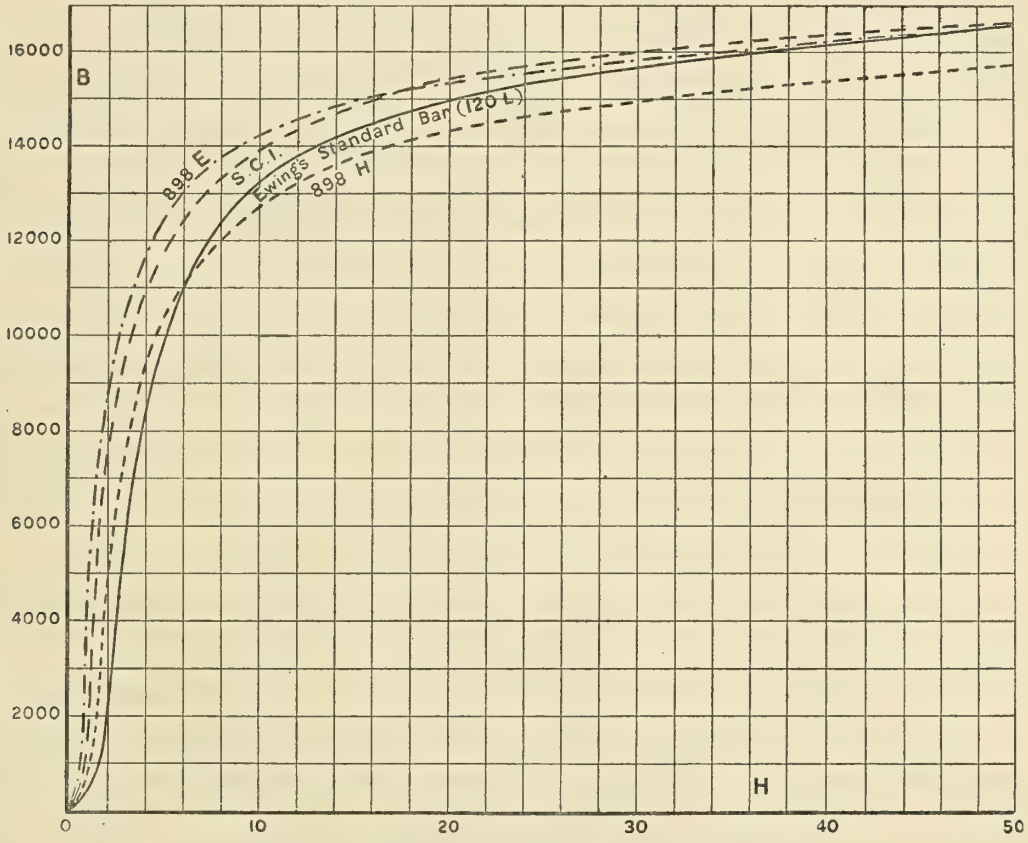


Fig. 2.— Permeability curves determined by Ewing's Bridge.

examine this matter more thoroughly the permeability of these silicon alloys was compared with that of our best and purest specimen of iron, S. C. I., by a wholly different method, employing for this purpose the new and ingenious apparatus devised by Professor Ewing, and known as his Permeability Bridge. The results are shown in fig. 2, where the curve marked 898 E. is a $2\frac{1}{2}$ per cent. silicon-iron alloy; 898 H is a $5\frac{1}{2}$ per cent. silicon-iron alloy, and 120 L is the standard curve supplied with the instrument of a specimen of good soft iron determined by Professor Ewing, and S. C. I. is our pure Swedish charcoal-iron, all the specimens being annealed. From these curves the following table of permeability, $\mu = B/H$, is obtained.

TABLE III.

COMPARISON OF MAGNETIC INDUCTION (B) AND PERMEABILITY (μ) IN THE PUREST SWEDISH CHARCOAL-IRON (S. C. I.) AND IN SILICON-IRON ALLOYS IN DIFFERENT FIELDS, AS DETERMINED BY EWING'S BRIDGE.

	S. C. I.		898 E. ($2\frac{1}{2}$ per cent. Si.)		898 H. ($5\frac{1}{2}$ per cent. Si.)	
H.	B.	μ	B.	μ	B.	μ
2·0	7,120	3,560	8,640	4,320	4,400	2,200
3·0	9,520	3,173	10,400	3,466	7,600	2,533
4·0	10,800	2,680	11,680	2,920	9,360	2,340
5·0	11,680	2,336	12,500	2,500	10,400	2,080
6·0	12,440	2,073	13,040	2,173	11,040	1,840
8·0	13,360	1,670	13,760	1,720	12,000	1,500
10·0	14,000	1,400	14,240	1,424	12,640	1,264

It will be seen that the magnetic permeability of the $2\frac{1}{2}$ per cent. silicon-iron is greater than iron up to a field of about 10 to 15 units. These results are so remarkable, and of such great practical importance, that it was desirable to test this conclusion still further. The specimens used in the Ewing's bridge were in the form of short cylindrical rods 18 cms. long, and 0·91 cms. diameter, coupled with a yoke of soft iron. The test rod is balanced against a standard rod by varying the strength of the magnetic field round it until the induced magnetism is the same in both rods, this equality being indicated by a magnetic needle; and the ratio of the permeability of the test rod to the standard is shown by the relative strength of the field required to bring about this equality. This arrangement does not enable the cyclic curve to be obtained, and consequently the relative hysteresis of the specimens cannot be found by it.

Another set of experiments were therefore made with the magnetometric method;

the long slender rods, S. C. I., 898 E, and H, were turned down to 0·4 cm. diameter and 102 cms. long. Unfortunately 898 H broke in the lathe, and hence in this third series of experiments it could not be compared with S. C. I. After being turned, both rods were carefully annealed by heating to whiteness in a gas-flame and cooling slowly in an east-and-west position. Annealing was also carried out by packing the rods in an iron tube with lime, heating to whiteness in a combustion furnace, and allowing the whole to cool slowly during the night: the magnetic measurements taken after this method of annealing did not, however, show much was gained in permeability. Plate II. shows the cyclic curves of the $2\frac{1}{2}$ per cent. silicon-iron alloy, 898 E, compared with that of our best iron S. C. I., both rods being 255 diameters in length; the correction for the reaction of the ends, estimated from Professor Ewing's experiments, is shown by the slope of the vertical axis: that is to say the value of the magnetising force H, required to produce any given induction B, must be estimated from the sloping line, and not from the vertical. The magnetometer employed was a delicate reflecting one, placed 70 cms. from the pole of the test rod, the needle being suspended by a fibre of unspun silk. It was found that the pivoted magnetometer employed in the earlier part of these researches (see Trans. R.D.S., vol. vii., p. 67) had become sluggish from wear, and a series of cyclic curves of these rods obtained by its means had to be rejected, as they proved to be untrustworthy.*

A fourth set of experiments for the purpose of testing this silicon-iron was made with rings of the alloy, using the ballistic method, undoubtedly the best but the most tedious. Castings were made of S. C. I. and 898 E; these were forged, and then annealed. The blocks were turned into rings in the Laboratory of the College of Science, and after turning raised to a bright red heat in sand, and allowed to cool slowly in the sand; another specimen of 898 E, after being turned into a ring, was sent to Sheffield for annealing. The dimensions of the rings were carefully determined and are given in Table IV., the cross-section

TABLE IV.

	Mean Diameter.	Cross sectional area.	Density.
S. C. I.	9·022 cms.	0·7299 sq. cms.	7·842
898 E	9·027 ,,	0·7649 ,,	7·665

* That is, the cyclic curves when plotted were all found to be flattened in the part where the induction changes rapidly with change of field; the relative value of the curves, however, was not much affected. Our experience shows that it is not safe to trust even the most delicate pivoted magnetometer in magnetic measurements where successive small changes of force require to be measured.

being similar to that of a curtain ring. The densities at 12° C. were also taken as shown in the last column.

The rings were each wound with a primary coil of 598 turns in three layers, and a secondary coil of 200 turns on the S. C. I. ring, and 190 turns on the 898 E ring. A Crompton bifilar ballistic galvanometer was employed, and the value of its deflections was determined by an Earth inductor as well as by a standardising coil. The Earth inductor was wound with 442 turns of insulated wire, the mean radius of its coils being 13 cms., the horizontal component of the Earth's field in which the inductor was placed was found to be 0·1714 in C. G. S. units; this value was checked several times. The standardising coil, which was used to check the calibration of the galvanometer by the Earth inductor, consisted of a uniformly wound solenoid 61·5 cms. long, having 2275 turns of wire on the outside, and in the middle of this primary coil a secondary coil of 200 turns of silk-covered wire was wound. The value of B per scale division thus found was slightly different for each ring, owing to their slightly different dimensions, &c.; each scale division of the galvanometer corresponded to an induction B, of 13·21 lines per sq. cm. for the S. C. I. ring, and 11·95 for the 898 E ring.

The result of a long series of experiments with these rings is given in the cyclic curves shown in Plate III. It will be seen that these curves closely resemble those in Plate II., obtained from the slender rods by the magnetometric rod. The specimen of silicon-iron was not quite the same in the two cases. The rod and rings were made from different castings which, upon analysis, were found to have the percentage composition given in the next table; the specimen of Swedish charcoal-iron, S. C. I., had exactly the same composition in both cases, and is here given for the sake of comparison. The iron is found by difference; traces of manganese, phosphorus, &c., were also present.

TABLE V.

	Si.	C.	Fe.
S. C. I., . .	0·07	0·03	99·9
898 E rod, . .	2·50	0·20	97·3
,, ring, .	2·87	0·20	96·9

These experiments afford an opportunity of testing the trustworthiness of the magnetometric method after applying the correction requisite for a rod of 255 diameters.

Here are the results for S. C. I. :—

TABLE VI.

DETERMINATION OF THE MAGNETIC INDUCTION (B) IN PURE IRON BY THE BALLISTIC METHOD (*ring*), COMPARED WITH THE MAGNETOMETRIC METHOD (*rod*, LENGTH = 255 DIAMETERS), THE LATTER CORRECTED FOR REACTION OF ENDS.

S. C. I.

	H.	B.		Difference.
		Ring.	Rod.	
Upgoing Curve	4	12,250	11,050	1,200
	6	13,600	13,000	600
	8	14,300	14,000	250
	10	14,700	14,700	0
	12	15,200	15,200	,,
	14	15,400	15,400	,,
	16	15,700	15,700	,,
	18	15,800	15,800	,,
Down Curve ...	60	17,600	17,600	,,
	18	16,200	16,200	,,
	16	16,100	16,100	,,
	14	16,000	16,000	,,
	12	15,900	15,900	,,
	10	15,700	15,700	,,
	8	15,500	15,200	300
	6	15,250	14,650	600
	4	14,800	13,600	1,200

It will be seen that in fields of ten units and over, the two results agree, but in the steep part of the curve, as the field gets lower, an increasing difference is perceived. This shows that either the correction is insufficient, at least for bodies of high permeability, in this part of the curve, or, much more probably, that the ring and rod were somewhat differently annealed, the ring being the better of the two, a difference which would reveal itself, as it does, in the lower magnetic fields.

From the cyclic curves in Plates II. and III., the hysteresis of the specimens can be compared; mere inspection shows that the area of the cyclic curve of the silicon-iron is considerably less than that of the Swedish charcoal-iron. From the areas of the curves the work done in going through a complete cycle can be estimated, and the hysteretic constant found. Another series of cyclic curves were obtained for a maximum induction B varying from 5000 to nearly 12,000, and the coercive force, remanence or residual induction, the hysteresis loss, or energy dissipated per cycle in ergs per c.c., and the hysteretic constant, η, were determined for each cycle. The results are given in Table VII., p. 12, and the upper part of the latter cyclic curves are shown in figs. 3 and 4 on next page.

TABLE VII.

MAGNETIC CONSTANTS OBTAINED FROM CYCLIC CURVES WITH RINGS.

ANNEALED SWEDISH CHARCOAL-IRON (S. C. I.).

Max. Induction in C. G. S.	Remanence.	Coercive Force.	Hysteresis Loss, Ergs per c.c.	Hysteretic Constant η.
17,700	10,800	1·10	8,754	·001397
11,970	9,090	0·95	3,572	·001067
9,907	7,850	0·91	2,732	·001104
4,044	3,975	0·72	943	·001158

ANNEALED SILICON-IRON (898 E).

Max. Induction in C. G. S.	Remanence.	Coercive Force.	Hysteresis Loss, Ergs per c.c.	Hysteretic Constant η.
17,500	8,000	0·80	6,730	·001094
11,260	7,375	0·78	2,710	·000892
9,440	6,600	0·70	2,000	·000873
5,079	3,850	0·54	741	·000872

UPPER PART OF CYCLIC CURVES OBTAINED WITH RINGS.

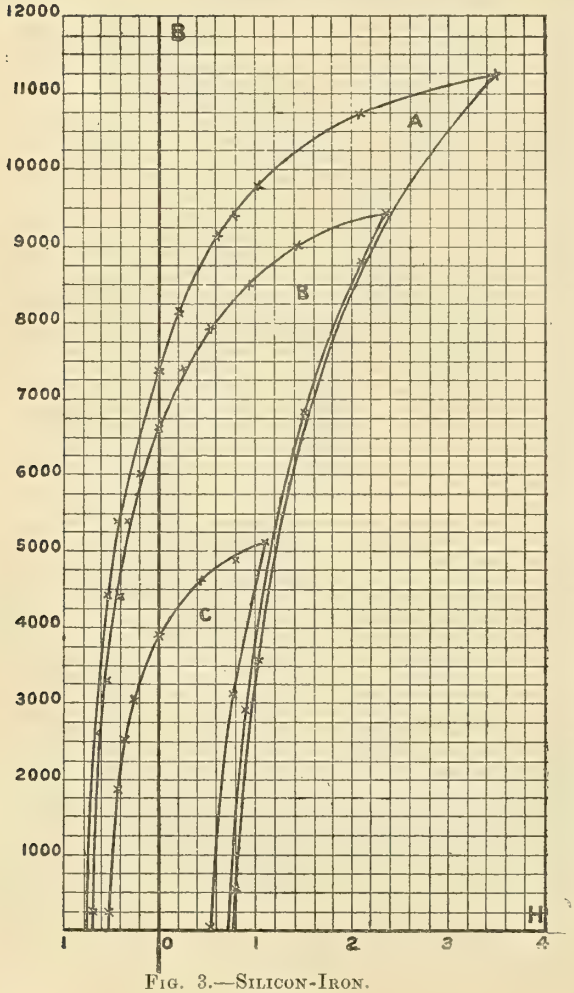


FIG. 3.—SILICON-IRON.

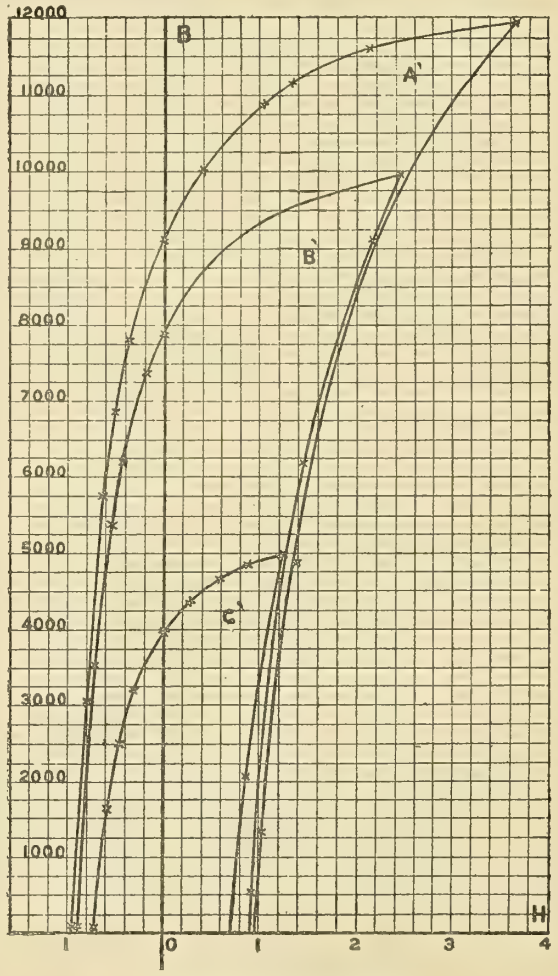


FIG. 4.—SWEDISH CHARCOAL-IRON.

In Table VIII. the hysteresis loss from reversals of a frequency of 100 is given in the commercial form of watts per lb. of material, the specimens being further compared with ordinary good soft iron, and a common maximum induction B of 5000 lines per sq. cm. is taken in each case.

TABLE VIII.

HYSTERESIS LOSSES IN S. C. I. AND 898 E COMPARED WITH GOOD SOFT IRON, FOR MAXIMUM INDUCTION **B** = 5,000 LINES PER SQ. CM., AND FREQUENCY n = 100, DEDUCED FROM FIGS. 3 & 4.

Mark.	Hysteretic Constant η	Hysteresis Losses.		
		Microwatts per c.c.	Watts.	
			Per cubic inch.	Per lb.
Soft Iron	0·002	16,576	0·272	0·9638
S. C. I.	0·0011	9,116	0·149	0·5266
898 E	0·0009	7,457	0·123	0·4415

As a maximum induction of from 4000 to 8000 is that usually employed in transformers, it will be seen that this silicon-iron is considerably better than Swedish charcoal-iron for the construction of transformers; the commercial value of this alloy is therefore great if its "ageing" quality is equally good. This latter point we have not yet fully determined, but experiments are in progress with small model transformers immersed in oil, which so far appear to indicate the extremely valuable magnetic properties of this alloy, and also of the low aluminium-iron alloy which is next discussed. Possibly an alloy having a small percentage of silicon and aluminium with iron, very free from carbon and manganese, will prove the best magnetic material for transformers and similar machines.

(3). *Aluminium-iron alloy.*

When iron is alloyed with a small percentage of aluminium, the magnetic permeability is increased to an even greater extent than in the case of the silicon-iron alloy. This discovery was made early in the course of our investigation, and was referred to in the paper we read before the Society on May 17, 1899.* The practical as well as the theoretic importance of this discovery made it

* Trans. Roy. Dub. Soc., vol. vii., p. 116.

desirable to submit the matter to more careful examination, and this we now proceed to describe.

The chemical composition of the Swedish charcoal-iron S. C. I. has already been given, it contains 99·9 per cent. of iron and had a density of 7·84. The aluminium-iron alloy, 1167 H, contains 97·3 per cent. of iron, 2·25 per cent. of aluminium, and 0·24 per cent. of carbon, and had a density of 7·56.

As was done in the case of the silicon-iron alloy, the specimen of aluminium-iron 1167 H, containing $2\frac{1}{4}$ per cent. of aluminium, was turned down to a

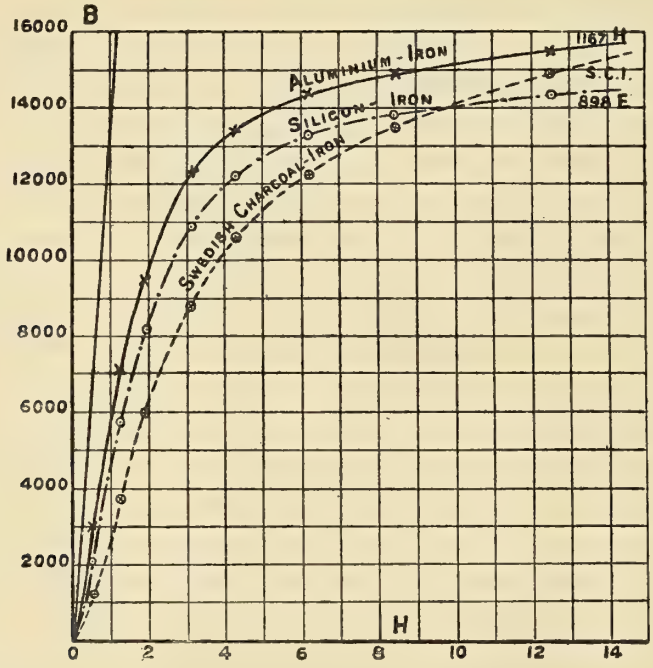


Fig. 5.—Permeability curves obtained from slender rods by the magnetometric method.

slender rod 0·3914 cm. diameter, its length remaining as before, 102 cms., the length was therefore 260 diameters. The rod was then annealed in the same way, and simultaneously with the similarly turned rods of silicon-iron and Swedish charcoal-iron.* The heat treatment of the three rods being exactly similar, they were then compared magnetically by the magnetometric method, the same reflecting magnetometer being employed as before, and described above. The magnetising solenoid, 20 cms. longer than the rods, was fixed vertically 70 cms. distant from the magnetometer.

The relative permeability of the rods was first determined, and the results,

* In the first instance the silicon-iron and pure iron rods were turned to 0·4 cm. diameter, but as the aluminium-iron when turned was found to be slightly smaller in diameter, the two former rods were now further reduced so as to make all three rods of exactly similar dimensions.

when plotted, gave the B-H curves shown in fig. 5, where the abscissæ are magnetising forces, and the ordinates the induction produced.

It will be seen that, in a field of 10 units, the silicon-iron alloy crosses the Swedish charcoal-iron, whereas the aluminium-iron retains its pre-eminence up to the highest field here employed. The experiments were several times repeated with the same results, the rods being heated to whiteness and cooled between each experiment. In fig. 5 the sloping vertical axis indicates the correction for the demagnetising reaction of the ends, estimated for rods of 260 diameters in length, according to Ewing's experiments on iron wires.* That is to say, in estimating the induction B in any given field, the magnetising force H must be reckoned from the sloping, and not the vertical, axis. This has been done in the accompanying table, which gives the induction B, and permeability, μ, of these rods in fields up to 10 units, deduced from the curves shown on a reduced scale in fig. 5.

TABLE IX.

PERMEABILITY OF SILICON-IRON (898 E) AND ALUMINIUM-IRON (1167 H) ALLOYS COMPARED WITH BEST ANNEALED SWEDISH CHARCOAL-IRON (S. C. I.), ALL IN THE FORM OF SLENDER RODS, LENGTH = 260 DIAMETERS.

	S. C. I.		898 E.		1,167 H.	
H.	B.	μ	B.	μ	B.	μ
2	7,400	3,700	10,200	5,100	12,000	6,000
4	11,150	2,790	12,300	3,075	13,800	3,450
6	12,600	2,100	13,400	2,233	14,500	2,416
8	13,600	1,700	13,800	1,725	14,900	1,862
10	14,400	1,440	14,200	1,420	15,200	1,520

The remarkable fact is here revealed that a permeability of 6000 is attained by the specimen 1167 H in a field of 2 units. But even this does not express the maximum permeability of the aluminium-iron alloy. In order to exhibit the permeability of this alloy, relatively to silicon-iron and Swedish charcoal-iron, the

* The extreme magnetic softness of the aluminium-iron may require a greater correction than is here shown. It would be desirable to have some experiments on this point with various lengths of aluminium-iron wire, or long ellipsoids, compared with ring experiments. At present, however, the only specimen of aluminium-iron alloy we have to work with is the one slender rod.

annexed curves, fig. 6, were drawn. Here the ordinates represent the permeability (B/H) instead of B, the abscissæ being magnetising forces as before.

These curves show that the aluminium-iron attains its maximum permeability in a field of about 0·5 unit, when it reaches the unprecedented value of 9000; the silicon-iron alloy has its maximum of 6400 in a field of 1 unit; and the Swedish charcoal-iron its maximum of about 4000 in a field of 1·5 unit. In fact, so magnetically soft are these alloys of aluminium- and silicon-iron, especially the former, that the vertical component of the Earth's magnetic force (equal 0·46 C. G. S. in Dublin) magnetises these alloys strongly when they are held vertically. Still more remarkable is the fact that, notwithstanding their length is 260 diameters, the mere reversal of the rods, without any jar or shake, instantly and completely reverses the magnetic polarity of the aluminium-iron and nearly reverses the silicon-iron, but only very partially reverses the pure soft iron S. C. I. which required slight tapping before complete magnetic reversal took place. This is shown by the accompanying table when the rods are compared under identical conditions.

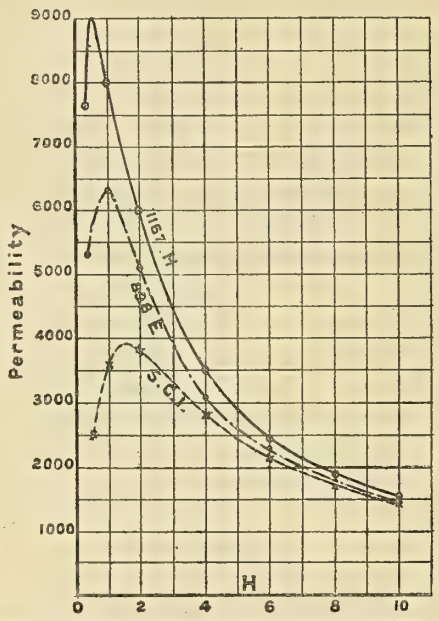


FIG. 6.—Curves showing Permeability in different fields of aluminium- and silicon-iron alloys compared with Swedish charcoal-iron.

TABLE X.

MAGNETOMETRIC DEFLECTIONS PRODUCED BY THE EARTH'S MAGNETIC FIELD ON SLENDER RODS OF—

Swedish Charcoal-Iron	35,	when rod reversed			15.
Silicon-Iron Alloy	68,	,,	,,	,,	58.
Aluminium-Iron Alloy	90,	,,	,,	,,	90.

Here the rods were, each in succession, held vertically at exactly the same distance from the reflecting magnetometer: the magnetometric deflections due to the Earth's magnetising force, are given in the second column; these being proportional to the tangents of the respective angles, therefore measure the relative permeability of the three rods in a magnetic field of about 0·5 C. G. S. unit. When the rods were carefully reversed without the least shock, so that the lower end occupied the precise position formerly held by the upper end, the deflections produced were in the same direction as before, showing the reversal of the magnetisation of the rods.

After the experiments, to be subsequently described, had been made, the

rod of aluminium-iron was drawn down to a long wire till its length became 452 diameters. Even at this, after careful annealing, the most gentle reversal of the wire almost completely reversed its magnetic state. Nothing could more strikingly illustrate the wonderful magnetic softness of this alloy, inasmuch as Professor Ewing has shown* that, when the length of a very soft iron rod is 400 to 500 diameters, the demagnetising reaction of the ends became negligible. That is to say, soft iron retains its magnetic state, like a ring of iron, when the magnetising force is removed: in our experiment, it is true, the action of the feeble magnetising force of the Earth is not removed from the rod, but reversed.

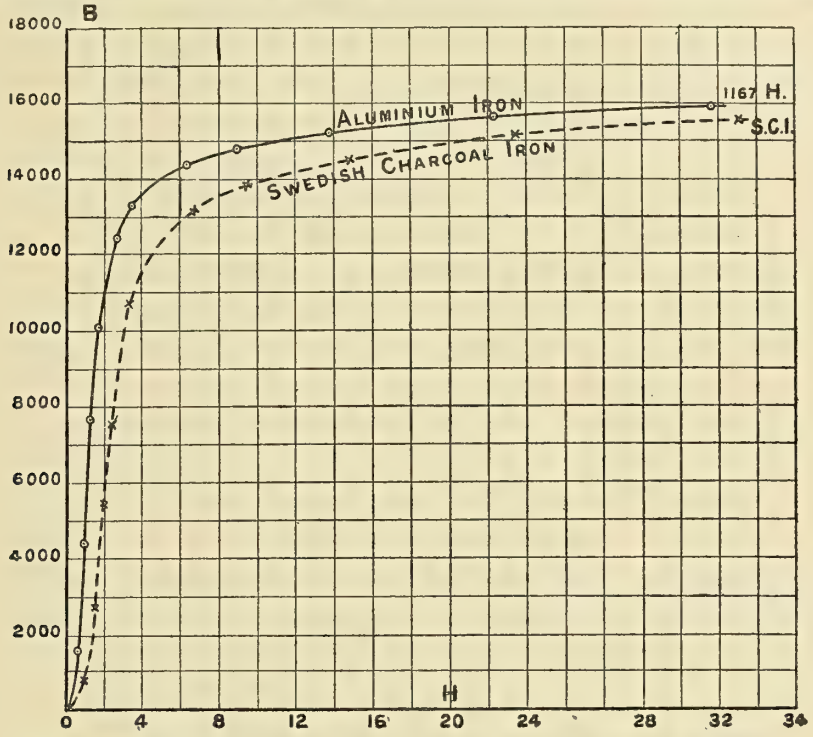


FIG. 7.—Permeability curves obtained from slender rods by the ballistic method.

It was convenient to use the vertical force of the Earth's field as the first step in the series of magnetising forces to which the rods were subjected in the experiments on permeability plotted in figures 5 and 6.† In the ballistic experiments next to be described, the rods could be placed horizontally, east-and-west, so that no step in the magnetisation was due to the Earth.

Both the long slender rods, S. C. I. and aluminium-iron, of 260 diameters in length, were tested ballistically by means of an excellent Crompton ballistic galvanometer. For this purpose a small secondary coil about 3·5 cm. long was wound on a thin copper sheath which could be slipped fairly tightly on to

* "Magnetic Induction in Iron and other Metals." New edition, page 34.

† In determining the cyclic curves to be shown later on, it was, of course, necessary to neutralize the magnetisation due to the Earth by a compensating current around the solenoid.

the middle part of the rod to be tested; this coil consisted of 400 turns of fine silk-covered copper wire in 4 layers. The rod and test coil were now put into the long magnetising helix (about 20 cms. longer than the rod) having a magnetising force = 30·2 C. G. S. units per ampère. The ends of the secondary coil were joined to the ballistic galvanometer through an Earth inductor in the usual way, and a series of steps taken in the same manner as in a ring test. The first parts of the B-H curves for the aluminium-iron alloy and the Swedish charcoal-iron are shown in fig. 7, and the values of the induction and permeability in different fields, deduced from fig. 7, are given in the next table.

TABLE XI.

RODS 260 DIAMETERS IN LENGTH TESTED BALLISTICALLY.

Swedish Charcoal Iron.			Aluminium-Iron Alloy.		
H.	B.	μ.	H.	B.	μ.
2	6,000	3,000	2	10,850	5,425
4	11,600	2,900	4	13,600	3,400
6	12,900	2,150	6	14,300	2,360
8	13,550	1,690	8	14,650	1,830
10	14,000	1,400	10	14,950	1,495

If this table be compared with Table IX. the results will be seen to be fairly concordant; but no correction has been made for the demagnetising reaction of the ends in the ballistic method; this correction would, of course, raise the above values of the induction and permeability.*

Before the ballistic experiments were made, the values of the hysteresis loss in the case of the two rods was found by a number of complete cycles determined magnetometrically with the following results.

The aluminium-iron rod of length = 260 diameters was put through a complete magnetic cycle up to a field of 60 units, and compared with a similar cycle obtained from the rod of Swedish charcoal-iron of the same dimensions, both having been subjected to identically the same heat treatment. The two cyclic curves are shown on Plate IV.; the sloping vertical axis indicating the correction for the demagnetising reaction of the ends. The aluminium-iron, 1167 H, shaded on the plate will be seen to have not only a higher permeability than S. C. I., but a greater

* If the same correction be made in both methods, the figures for the ballistic method appear to be somewhat high; indicating either that a lower correcting factor is necessary, or that the inductance per scale division of the galvanometer has been over-estimated in this method.

induction in all fields up to the highest here tried, viz. 60 units: from the relative area of the two curves the hysteresis loss will also be seen to be less. The values of the magnetic constants of the two substances deduced from these curves are as follows:—

TABLE XII.

MAGNETIC CONSTANTS OBTAINED WITH SLENDER RODS TESTED MAGNETOMETRICALLY.

Mark.	Magnetising force.	Maximum Induction.	Remanence.	Coercive Force.	Hysteresis loss per cycle.
S. C. I.	60	17,750	8,250	1·1	8,230 ergs.
1167 H	60	18,250	11,000	0·6	5,270 „

It will be noticed that the coercive force of the aluminium-iron is little more than one-half, and the hysteresis loss only five-eighths of the Swedish charcoal-iron, albeit its maximum induction is somewhat higher. The remanence, reckoned,

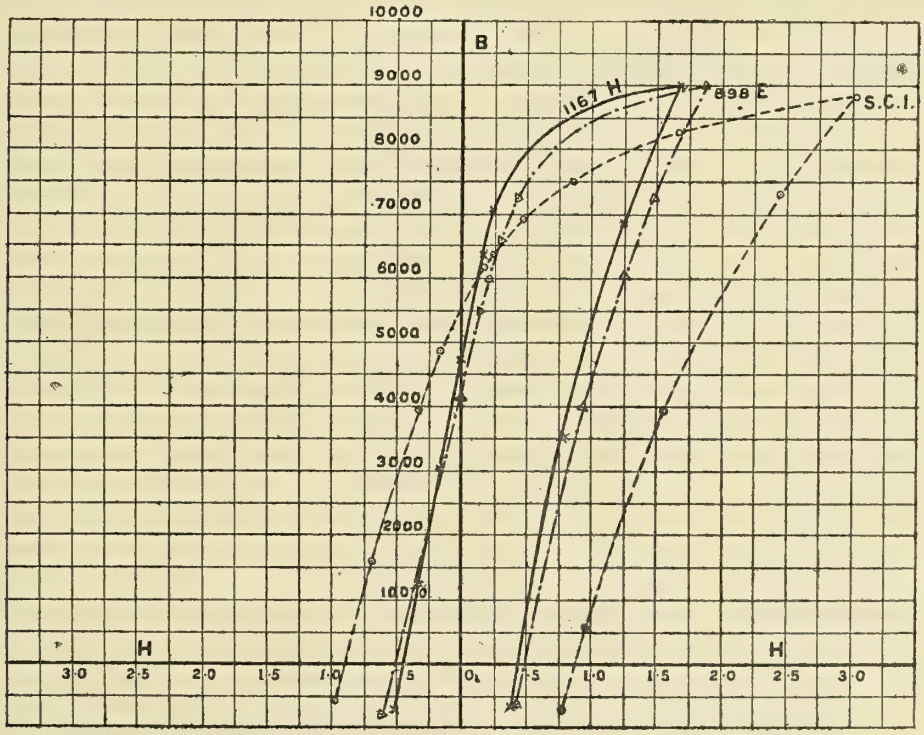


FIG. 8.—Cyclic curves obtained from slender rods of aluminium-iron and silicon-iron alloy compared with Swedish charcoal-iron.

of course, from the point where the sloping axis is cut, is seen to be higher in the aluminium-iron; this is unexpected, but experiments with the ring of S. C. I. gave a higher remanence than with the above rod (see Table VII., p. 413). The

magnetic difference between the silicon-iron and aluminium-iron is seen at once by comparing Plates III. and IV. Up to this we have had no ring of aluminium-iron so that the above results can only be considered as comparative. If we take the figures obtained from the rings of Swedish charcoal-iron and silicon-iron, we see, from Tables VII. and XII., that, at a magnetising force of about 60 units, the maximum induction, coercive force, and hysteresis loss of silicon-iron (898 E) comes between S. C. I. and 1167 H.*

It seemed desirable to make a more strictly comparative experiment of the relative values of these three substances at a maximum induction of about 9,000, which is the highest limit of the induction usually employed in transformers. The rods were therefore re-annealed simultaneously; and the cycles shown on fig. 8, on previous page, were taken by the magnetometric method. From these curves the following magnetic constants were deduced, and for the sake of comparison the results usually given for good soft iron are included :—

TABLE XIII.

HYSTERESIS LOSSES FOR MAXIMUM INDUCTION = 9,000 C. G. S. AND FREQUENCY = 100 OF RODS OF SILICON-IRON (898) AND ALUMINIUM-IRON (1167) COMPARED WITH ORDINARY AND PURE SOFT IRON (S. C. I.) ALL ANNEALED.

Mark.	Density.	Hysteretic Constant	Hysteresis Losses.		
			Microwatts per c.c.	Watts.	
				Per cubic inch.	Per lb.
Soft Iron	7·78	·002	42,440	0·695	2·50
S. C. I.	7·84	·0011	23,340	0·382	1·35
898 E.	7·66	·00073	15,490	0·254	0·92
1167 H.	7·56	·00068	14,430	0·236	0·86

The energy dissipated is given in watts per cubic inch of material, and also per lb. per 100 cycles; and it will be seen that at this lower induction the aluminium-iron is considerably better than our purest specimen of iron. It is certainly an extraordinary fact that, adding $2\frac{1}{2}$ per cent. of a non-magnetic element to iron, should confer upon the iron a higher magnetic quality. This is probably due to the increased mechanical softness given by alloying iron with aluminium.

* The character of the cyclic-curve for 1167 H is somewhat peculiar; it swells out at the bend more than the S. C. I., and contracts more rapidly than the iron as the horizontal axis is approached.

We have recently received from Messrs. J. Sankey and Son, of Bilston, a specimen of the "Lohy" iron that they manufacture for transformers, and which has a hysteresis loss of only 0·32 watts per lb. for a maximum induction = 4000 C. G. S., and a frequency of 100 cycles per second.

The results obtained with our silicon-iron and aluminium-iron alloys when carefully annealed are considerably better than the "Lohy" iron at this maximum induction and frequency. The figures are as follows:—

S. C. I., ...	0·38 watts per lb.
"Lohy" Iron, ...	0·32 ,, ,, ,,
898 E, ...	0·26 ,, ,, ,,
1167 H, ...	0·23 ,, ,, ,,

In conclusion, the question remains to be considered what is the chemical or physical change produced in iron by the presence of other metals, so that on the one hand the magnetic permeability of the alloy is practically destroyed by the added element, and on the other increased. From their strong chemical affinities for oxygen and the halogens, silicon and aluminium may act both chemically and physically. By combining with oxygen or with dissociated gas they may render the iron freer from oxide of iron, or give it a closer texture and prevent fine honeycombs being formed. Microscopic examination of sections of all these alloys is needed; such examination cannot fail to throw light on the relation between magnetic qualities and the size and nature of the grain, or the crystalline structure of the alloys. Experiments also need to be made on the recalescent points or critical temperatures of these alloys, and on the question whether part of the iron, or of other metals entering into these alloys, is in an allotropic state. The condition in which the carbon exists in the alloy is also a matter of great importance magnetically as well as mechanically. The change in the magnetic quality of iron and iron alloys by annealing no doubt largely consists in the alteration in the state of the carbon, as well as of the iron, at the temperature of recalescence.*

As magnetisation involves a molecular change in magnetic bodies, whatever affects the state of molecular aggregation in these bodies affects their magnetic susceptibility. An increase or decrease of mechanical hardness means an increase or decrease of the power of resisting molecular change; and hence magnetic hardness or high coercive force, and magnetic softness or high permeability, correspond to the mechanical hardness or softness of iron and its alloys. It is thus easy to understand why a body which in the annealed state is feebly magnetic in a certain magnetic field becomes non-magnetic in the same field

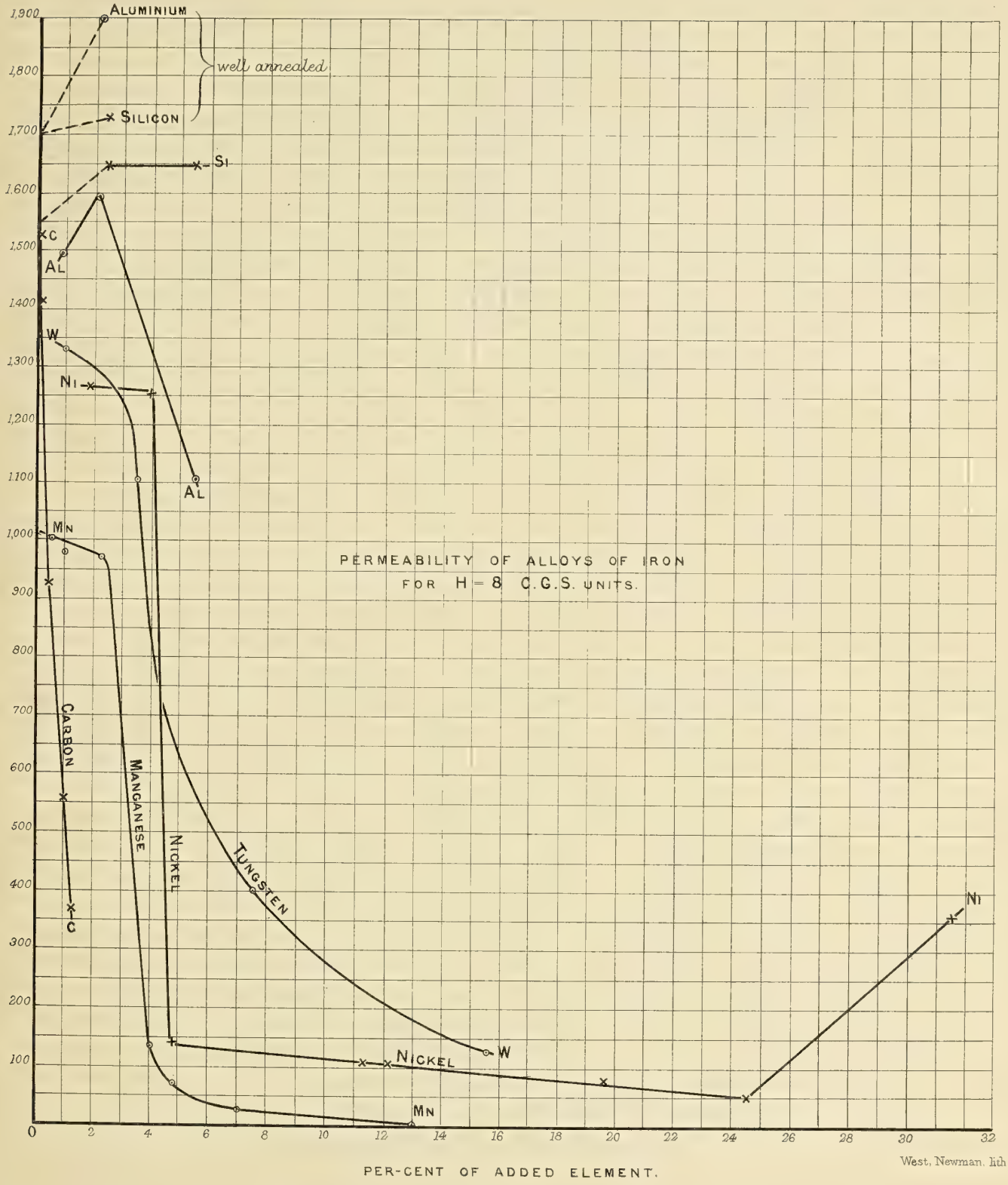
* In this connexion Herr Kamps has some valuable observations in his second Paper in *Stahl und Eisen* for October, 1899.

when unannealed. By increasing the strength of the field the resistance to molecular change may be partially overcome, and the unannealed body thereby made more or less magnetic. The conversion of a magnetic into a non-magnetic body by alloying the former with certain metals may, therefore, largely depend upon the increase in mechanical hardness of the alloy. And conversely the increase in permeability which silicon and aluminium confer upon iron and steel is probably due to the greater mechanical as well as magnetic softness of the alloy.

We desire to express our appreciation of the assistance Mr. S. A. Edmonds, a Senior Student in the Physics Faculty of the College of Science for Ireland, has rendered us in some of the numerous observations and plotting of curves given in this part of our investigation.

P.S.—Experiments are now in progress with a ring of the aluminium-iron alloy (only recently obtained) with which the cyclic curve and hysteresis loss can be more accurately determined.* The high permeability of this alloy would doubtless be still further increased by reducing the carbon it contains and by a slight alteration in the amount of aluminium present, and also by careful attention to the heat treatment of the alloy.

* *Note added later, June, 1902.* The specimen of aluminium-iron of which the ring was made has not given at all such good results as the rod of this alloy which was first received, and with which the determinations given in the foregoing memoir were made. The permeability and hysteresis loss of the ring were found to be *inferior* to Swedish charcoal-iron. This may arise from some difference in heat treatment—which is now being tested—or more probably to a slight difference in the chemical composition of this specimen, arising from another casting, or to segregation occurring in some portion of the casting, or possibly to some other undiscovered cause. It is desirable to draw attention to this fluctuation in the magnetic character of the aluminium alloy, for—although no doubt at all remains in our mind of the accuracy of the comparative results given in the foregoing memoir, the experiments speak for themselves,—samples of aluminium with alloy may be placed on the market which may be found not to give the high magnetic quality we have proved to exist in the particular specimen we employed. A careful investigation is now being made with a view to the discovery of the cause of this anomaly.



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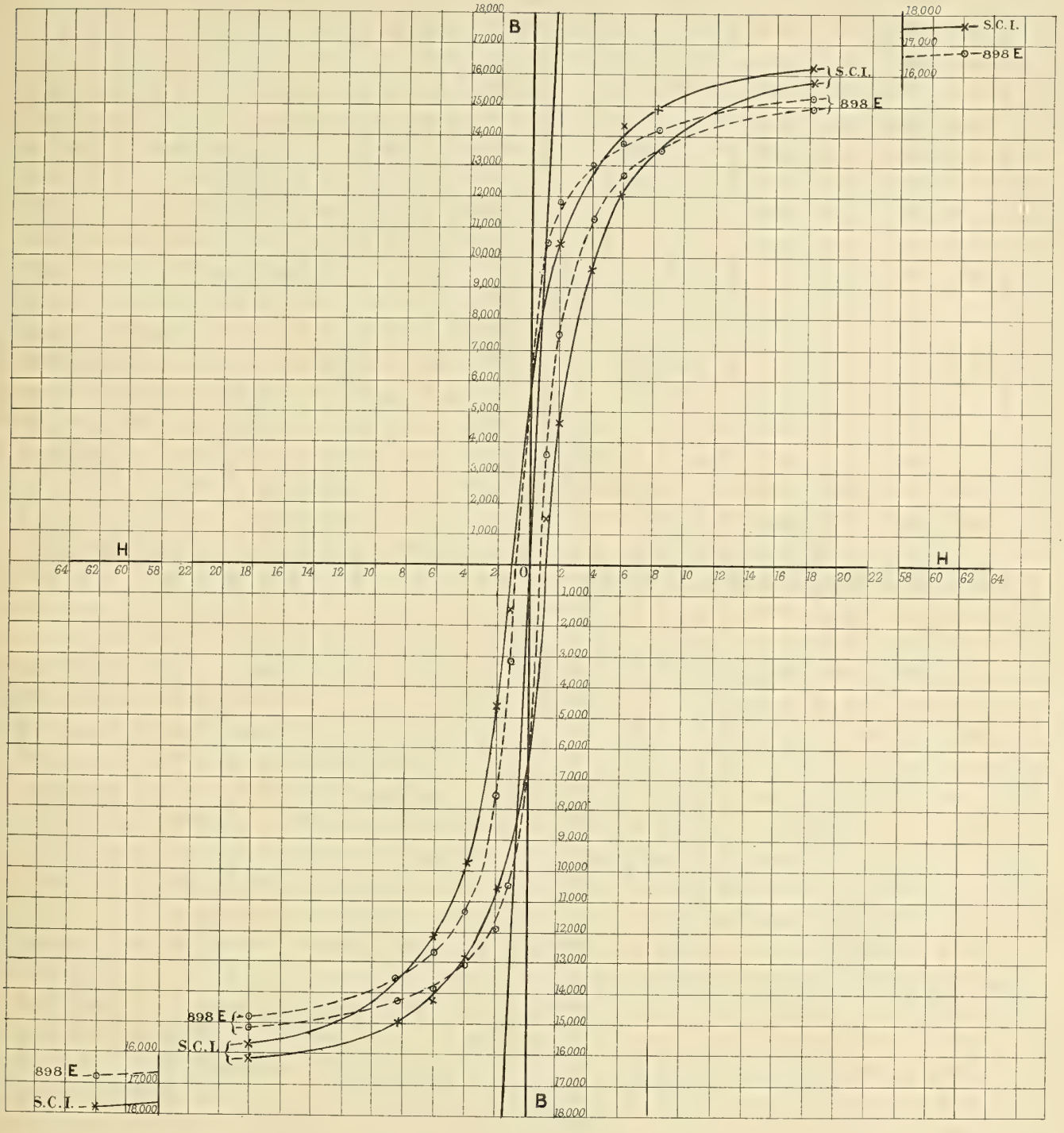

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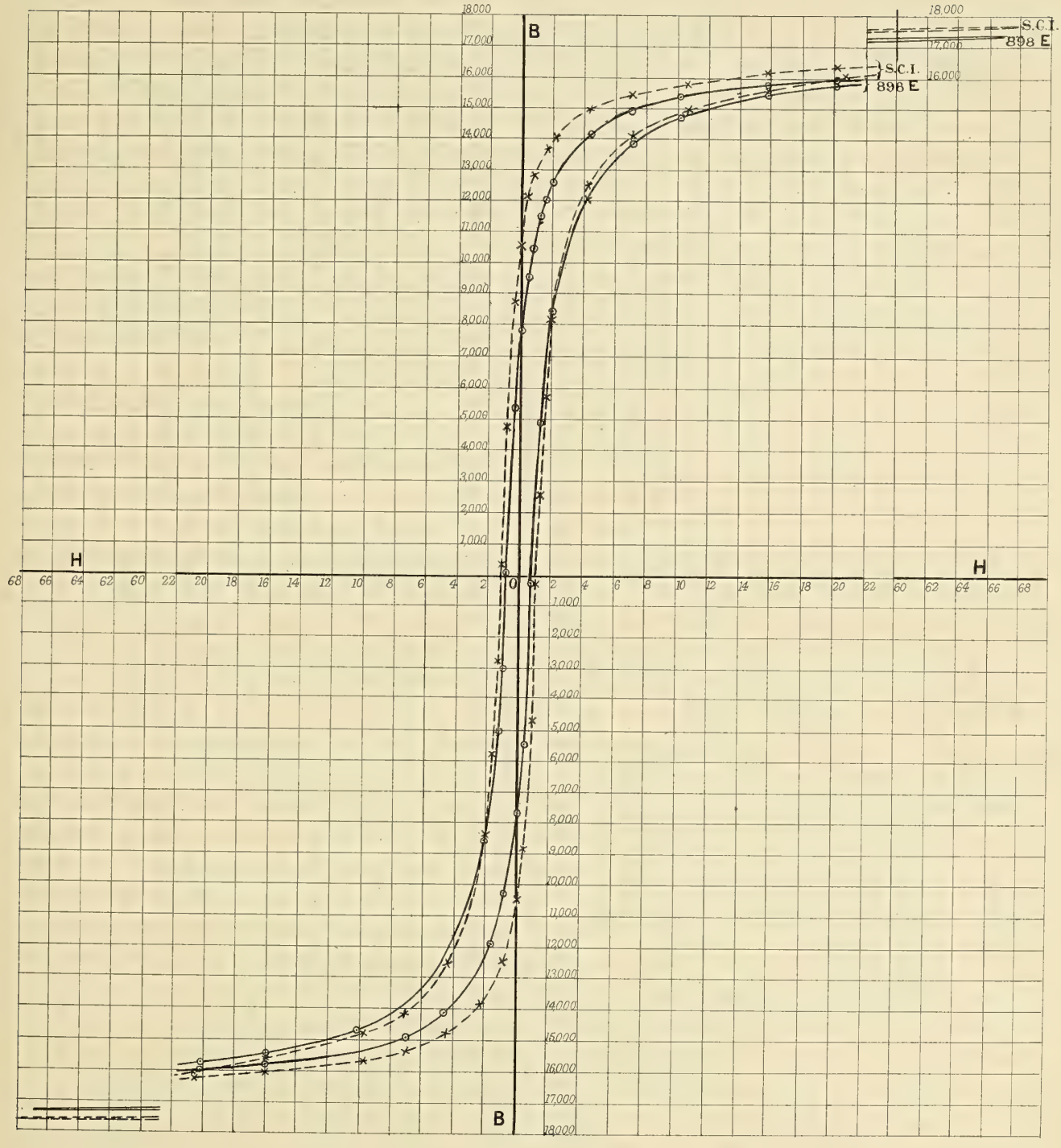
CYCLIC CURVES FROM RODS (LENGTH- 255 DIAMETERS) OF SILICON IRON AND SWEDISH CHARCOAL IRON.



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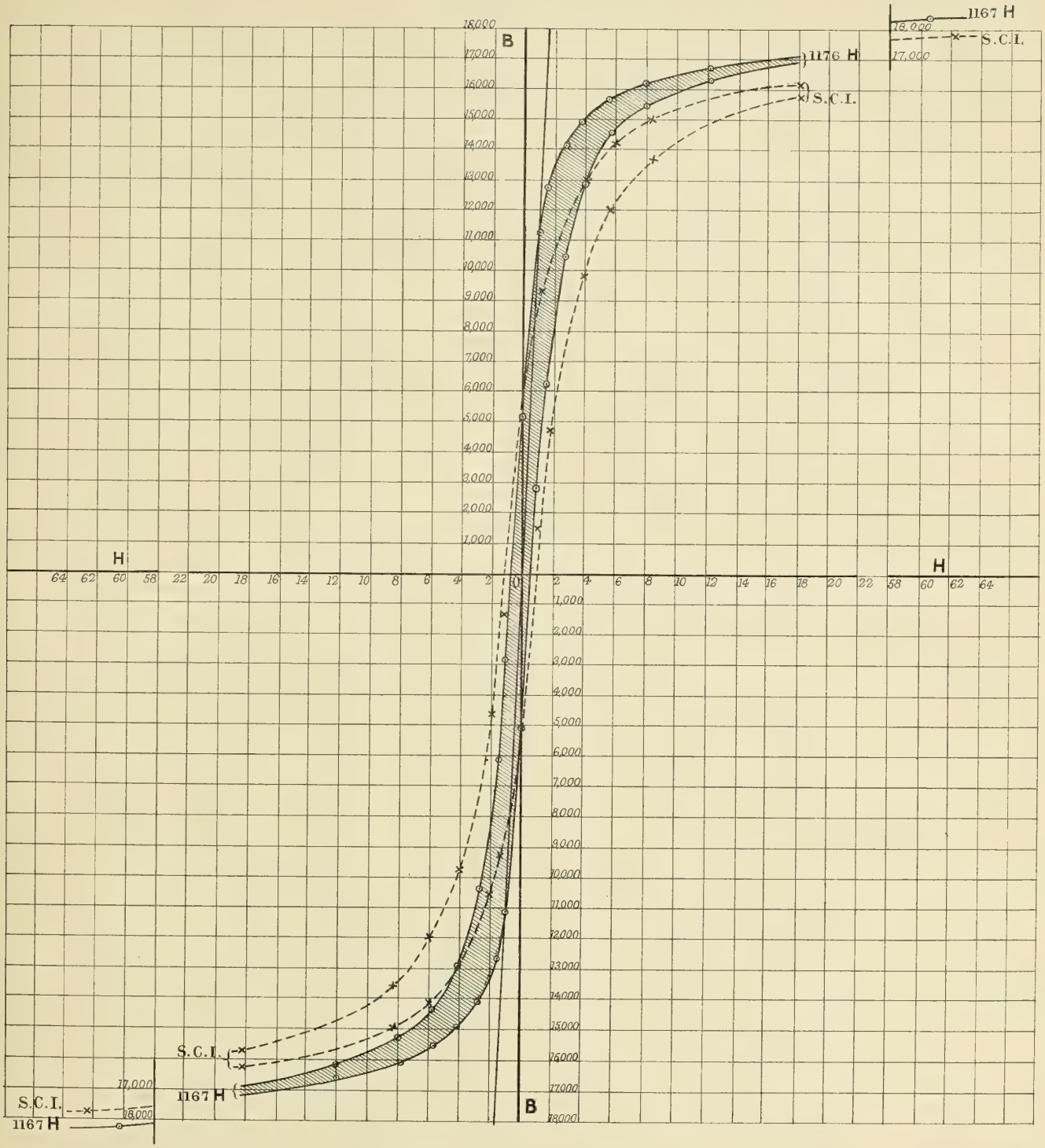
CYCLIC CURVES FROM RINGS OF SWEDISH CHARCOAL IRON AND SILICON IRON.



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CYCLIC CURVES FROM RODS (LENGTH-260 DIAMETERS) OF ALUMINIUM IRON AND SWEDISH CHARCOAL IRON.

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Vol. V.—Parts 1-13.—May, 1893, to July, 1896.
Vol. VI.—Parts 1-16.—February, 1896, to August, 1898.
Vol. VII.—Parts 1-16.—August, 1898, to September, 1902.

VOLUME VIII.

Part

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[October, 1903.]

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ON THE CONSERVATION OF MASS.

By J. JOLY, D.Sc., F.R.S., F.G.S., Professor of Geology in the University of Dublin, Hon. Sec., Royal Dublin Society.

(Plate V.)

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The recent publication of a memoir by A. Heydweiller, "Ueber Gewichtsänderungen bei chemischer und physikalischer Umsetzung,"* has again raised the question of a possible alteration of weight attending certain chemical and physical changes. The paper has been closely criticised; but up to the present no commonplace explanation of the results has been suggested. The most serious criticism is to be found in the dynamical difficulties which have been shown to attend the acceptance of the conclusion arrived at by the experiments.†

As to the experimental details of Heydweiller's work, the reader of his paper feels inclined to ask why so many possible sources of error were left to evaluation and not removed by suitable arrangement of the apparatus. The density and hygrometric state of the atmosphere are capable of elimination as factors in comparative experiments of the kind by exhaustion of air from the tubes containing the reacting substances, air-tight closing of the balance and proper drying of the contained air. The necessity of handling the tubes is again avoidable; although Heydweiller's balance did not, as he states, permit of arrangements to effect this. But while it is possible to plan experiments of a more direct character, we cannot on this account refuse to attach value to Heydweiller's results. It is difficult to suggest how a systematic error such as would be required to account for his results, can have entered his experiments.

A large part of the little we know of the physics of mass and weight may be comprised in the statement that the masses of bodies are proportional to their gravities. If this is not strictly true, or if the connexion breaks down under certain conditions of intermolecular actions, we are apparently at liberty to speculate on many far-reaching consequences. It must be admitted that some of the most remarkable of these are not contrary to our experience when

* Annalen der Physik, Bd. 5, 1901, 394. † Lord Rayleigh in *Nature*, vol. lxvi., p. 58.

closely examined. The connexion between mass and weight has not hitherto been closely traced through chemical changes on the one hand; and on the other there is nothing in our experience to negative the existence of non-gravitational mass in the universe. The organising property of gravitation being wanting in such stuff, it must necessarily become distributed in space, and so may acquire a rarity rendering it without influence on our observations of whatever kind. If, however, the connexion between mass and weight is inherent in the nature of matter, then we have to consider certain consequences which may be expected to attend a weight-change, and which may be capable of detection experimentally, besides loss of gravitational attraction.

These consequences are such as would attend the disappearance of mass from matter progressing through space with the velocity which, as part of the Earth, it possesses. The reverse problem is, perhaps, more simply considered. What effect must attend increased inertia or the creation of mass under such conditions? There are many difficulties in the way of assuming the required energy supplied from the ether. Energy is certainly required to confer upon the new mass the prevailing velocity. The immediate conclusion would be that the interacting substances (within which we assume the change to occur) would share their directed kinetic energy with the new mass. On the other hand, if mass disappears, the energy associated with its inertia is apparently available. As this is directed energy, there is difficulty in assuming it to be dissipated entirely in undirected forms, leaving no resultant acceleration, in the direction of motion of the remaining mass.

In short, if the weight-force disappears, and a fundamental interdependence of weight and mass obtains, a change in the inertia of the body must attend the loss of weight. The change would be one of diminution, it is to be presumed, if the weight-change is one of diminution. We may then ask: does a mass-change attend such physical and chemical reactions as formed the subject of Heydweiller's experiments? We have apparently no way of detecting such mass-change, unless it be attended by gross mechanical effects upon the reacting substances traceable to the kinetic energy and momentum associated with the disappearing inertia. I am well aware of the obscurity attending the effects to be investigated, and how many possible hypotheses, unhampered as we now are by facts, may be framed to evade the necessity of assuming any such gross mechanical motions. Even so, exploratory experiments are justified, and are still justified even were the commonplace explanation of all hitherto recorded weight-loss experiments forthcoming. Indeed, the interdependence of mass and gravitation under conditions of molecular rearrangements has been so scantily explored that the possibility of inertia effects arising unattended by gravitation effects must not be lost sight of. If inertia is a phenomenon wholly or in part referable to

convection of electrons in the ether,* inertia effects might possibly attend change in state of molecular aggregation. This, of course, offers no explanation of such results as Herr Heydweiller's.

If the original mass is M grammes and the final mass $M - m$ grammes, and both possess a velocity v (say, the orbital velocity of the Earth), then the kinetic energy is greater, of course, for the body M than for the body $M - m$. In order that the kinetic energy should be conserved, we have to assume that the velocity v becomes increased to the velocity V, and so that $Mv^2 = (M - m)V^2$.

From this

$$(V + v)(V - v) = \left(\frac{m}{M - m}\right)v^2;$$

and writing $2v$ for $V + v$, and M for $M - m$, we have

$$V - v = \frac{mv}{2M}.$$

If conservation of *momentum* be looked to, the similar equation will lead to a velocity change double of that just obtained.

If experiment results in showing no change in velocity—in other words, no gross dynamical effect—then, as stated above, a variety of hypotheses may be advanced to reconcile a true or apparent weight-loss with an apparently unaltered inertia. Some reference may be made here to one such speculation.

It may be suggested that the loss of weight observed in Heydweiller's experiments is due to loss of electrons, which carry with them both weight and mass; necessarily resulting in an apparent (but not real) weight-loss and perfect constancy of momentum in such experiments as I am describing. Such a process as is here supposed would resemble diffusion, and probably would not be reversible. There is no evidence, however, to negative the idea that by repeated solution of, say, copper sulphate in water and its recrystallization, the entire mass, or some constituent of it, can become dissipated. If this is the explanation of Heydweiller's results, so considerable a radiation of electrons should reveal itself in physical effects prevailing in the proximity of the reaction. Such an explanation leaves the interdependence of mass and weight unquestioned.

The velocity of the Earth in its orbit is $2{\cdot}95 \times 10^6$ cms. per second, and the velocity of a point in the equator due to diurnal rotation 46×10^3 cms. per second. At the latitude of 53° the diurnal velocity is $2{\cdot}74 \times 10^4$ cms. per second. Thus in this latitude we have at noon a velocity, east to west, of the orbital less the diurnal, at midnight a velocity due to the sum of these motions, and as regards objects on the Earth's surface directed west to east. The noon and midnight velocities may be taken as alike for present purposes, and as about 3×10^6 cms. per second. The

* Professor J. J. Thomson, *Phil. Mag.*, April, 1881.

fact that the velocities are oppositely directed is of much value, obviously, in discriminating among true and spurious effects. We have further the facts that at 6 o'clock in the afternoon and at the same hour in the morning the velocity is but one per cent. of the mid-day velocity, and directed west to east. The solar drift towards Hercules is estimated to be as much as 246×10^4 cms. per second,* and is directed northward at a high angle, about 55°, with the Ecliptic. Much lower values for the velocity are, however, sometimes given. The effect will be to increase the space-velocity of the Earth in spring-time and diminish it in autumn, and that by an amount which may be very considerable. In the estimates which follow, I leave the solar drift out of account.

To arrive at an idea of the magnitude of the effects we are exploring, we may refer to Heydweiller's results as a basis. Take those on the solution of ordinary copper sulphate in water. The total reacting masses amount to 209 grammes, of which 0·126 mgr. disappears, or, say, 0·1 mgr. in 200 grms. But the process of solution extends necessarily over a certain duration of time. Suppose we powder the solid so finely that the added water becomes saturated almost as fast as it reaches the solid, and that the time of flow is 60 seconds. In this case, then, the mass disappearing per second is

$$\frac{0{\cdot}0001}{60} = 1{\cdot}7 \times 10^{-6} \text{ grms.}$$

Now, on the assumption that

$$V - v = \frac{mv}{2M},$$

i.e. that the directed kinetic energy is conserved as such, we have

$$\frac{1{\cdot}7 \times 10^{-6} \times 3 \times 10^6}{2 \times 200} = 1{\cdot}27 \times 10^{-2} \text{ cms. per second.}$$

This is the acceleration acting during the first 60 seconds. The corresponding accelerating force is 2·54 dynes. If we now assume that the mode of observation involves the use of a counterpoise of equal mass to the reacting substances, and that the necessary containing vessels and their counterpoising masses amount to 50 grms. more, the total mass which has to be accelerated by the force derived as above is 450 grms. If the motion is rectilinear and without opposing resistance, the acceleration of the total mass will be $5{\cdot}6 \times 10^{-3}$ cms. per second per second, and the space described during the 60 seconds will be a little over 10 cms.

This same large effect, derived on the basis of the supposed weight-loss, is obtained with less or greater quantities of reacting substance, if the mass which it is necessary to confer on the containing vessel is proportional to the mass of active

* See G. H. Knibbs, Proc. R. S., New South Wales, 1900, vol. xxxiv., p. 148, *et seq.*

material; in other words, the result is independent of the mass of active substance taken. The experiment may therefore be carried out on a large or small scale, as other circumstances may dictate.

In the foregoing calculation, the conditions are such as would obtain with the use of a torsion-balance, the active substances being carried on a beam of considerable length. We may seek for the inertia-effect in other ways. Thus obviously the use of a sensitive chemical balance, observed during the reaction, and at such times that the meridian circle of the observer is approximately tangential to the Earth's orbit, should reveal a temporary apparent change of weight. The benefit of a continued acceleration would be lost, however, and the method exposed to errors of temperature in an exaggerated degree, as well as to errors arising from loss or gain of material. Errors from the latter source do not affect the torsion-balance method. Again, the reacting substances may be enclosed in the bob of a sensitive pendulum. This method has the great advantage of not requiring the added inertia of the counterpoise. On the other hand, however, the range of motion is so small under the constraining effect of the weight-force attending deflection that, with any reasonable length of suspension, tremors of small magnitude arising from external sources or from the internal motions attending the mixing of the chemicals would mask the true effects. After full consideration the torsion-balance method was adopted in the experiments to be now described.

The arrangements at first made were modified in several particulars as difficulties arose. It is needless to do more than refer to those now in use, and by which the results given in this paper have been obtained.

The torsion-beam is constructed of straws; taking the form of a king-post truss (the king-post being above the horizontal member). The component straws meet in corks to which they are attached by glue. The horizontal member is 25 cms. long; the king-post 10 cms. The weight of the beam, including a galvanometer-mirror, is 2·5 grammes. This beam carries 100 grammes at each extremity without serious flexure. The suspending wire is of phosphor-bronze, ribbon-section. Its breaking weight is 355 grammes. It is attached by soft solder to a wire which passes axially right through the king-post and diametrally through the horizontal member of the beam.

After trying various lengths of suspension, finally a length of 30 cms. was adopted. In some respects a still shorter one would be desirable. Owing to the bulky nature of the suspended vessel, convection currents are particularly trying; and with this length of suspension, a zero position is never assumed, and indeed is not accurately determinable. Somewhat greater restraint arising from the torsion moment of the suspension would not affect the limits of accuracy obtainable in practice.

The method adopted finally for bringing the reacting substances together will best be understood from the figure, Plate V., which is approximately to natural scale. The vessels *a* and *b* are of lightest blown glass fused together. The upper holds the liquid substance; the lower the solid. The capacity of *a* is about 30 c.cs., of *b* about 50 c.cs.; the solid is introduced into the lower vessel by means of the lateral tubule. The liquid is drawn into *a* by temporarily inserting a cork in this tubule perforated by a fine-drawn glass tube to which a rubber drainage-tube is affixed, and sucking on this while the tubule x is inserted in the measured quantity of fluid. This may be effected after the solid constituent has been inserted into *b*. The fine-drawn glass tube shown in the figure is then affixed between the tubule and x, the vessel being held approximately horizontal during the operation. This fine tube is not open throughout its length, but is stopped at the bend *q* by a *very* short plug of low melting-point substance. There is no difficulty in slipping the one extremity into the rubber cap fitted on x, and the cork into the tubule. The operation is easily effected without spilling a drop of the liquid. Placed finally in the upright position shown in the figure, it is evident that the melting of the plug at *q* will allow the liquid in *a* to gravitate gently into *b*, the air displaced from *a* circulating through the tube. To ensure that no air escapes or is sucked in, it is necessary to seal up the cork fitting the tubule with wax, and to varnish the rubber connexion at x with the same or other air-tight substance. The filled vessels are next placed in an outer paper cylinder, and very fine dry cork-dust (prepared by filing down cork of good quality) poured in around them. The cylinder is freely suspended by fine copper wire from the extremity of the beam.

The liberation of the liquid carried in the upper bulb is effected by radiation from a short ribbon of platinum (through which a current may be passed) over which the bend of the tube *q* oscillates in its vibrations about the zero position of the beam. This platinum radiator is contained in a separate compartment to the front of the case enclosing the beam. The fine glass tube enters this cell through a slit between glass plates, and may be observed through an external window, so that the break-down of the plug may be assured. The necessity of the cell is obvious: disturbance of the air within the body of the case appears to be completely avoided or delayed by its use to such an extent that even trials of the radiator for periods much longer than those required to melt the plug produced no discoverable effect. The time actually required to melt the plug varies from one to three seconds, the platinum being raised to a visible red. The low melting-point substance consisted of a mixture of vaseline and paraffin melting at 27·5° C. It might appear as if much difficulty would be experienced in getting the plug into the correct position in the flattened bend of the long, fine tube. But this is not so. In fact, a very little of the vaseline mixture

being run into the warmed tube, this quantity can be chased about the tube by waving it over a low flame, into any required position. The quantity used must be no more than will give a plug at the bend of a length hardly greater than the internal diameter of the tube. Otherwise there is risk that the plug on heating will not break down, but simply shift its position. Or the plug may be placed in the rectangular bend most remote from the upper vessel, when a very small quantity suffices.

The foregoing arrangement for bringing the reacting substance together has worked most satisfactorily. The method which would at first suggest itself would be to use the heating effect of a current conveyed to the vessels along the beam, and entering at mercury contacts at the centre of the beam. This was actually tried and used for some time. In this case, the fusible plug was carried in the connecting tube just above x, and was melted by a current circulating in a platinum wire surrounding it. None of the results given later are obtained with this arrangement. It was found that the effects of the contacts were not constant, but depended upon the cleanliness of the mercury surfaces to a degree which introduced a very variable resistance to the swinging of the beam. A perfect adjustment of the iron wire points entering the mercury cups beneath the centre of the beam, so that there was perfect alignment with the suspension, would doubtless have permitted of make and break of contact without disturbance of the vibrations; but such alignment was never attained, and evidently would be most difficult to arrive at. It being found difficult to effect make and break without perturbations, the contacts had to be left on during experiment; and, as I have stated, a variable resistance or friction introduced thereby, which finally led to the abandonment of the arrangement.

The counterpoise carried at the other extremity of the beam is in the form of a narrow test-tube filled to a certain depth with mercury, to which more was added or from which some was taken according to the mass of active substances used in the experiment.

The beam and its load, as described, are contained in a strongly made case of pine, lined with zinc, and provided with a removable front. This front is double-walled, the internal thickness being about 4·5 cms. The space between the walls is filled with fine, dry shavings of wood. A cell is, however, constructed in the thickness of the removable front, in which the platinum heater, as already described, is located. This cell is closed in front by a glass window, and on the inner side is also glazed; but with a horizontal slit left for the entry of the air-tube connecting the vessels as referred to above. The side of the wooden case is furnished with another window, 5 × 10 cms., for the admission of the rays required to form an image, after reflection from the torsion-mirror, upon a divided scale; the torsion-wire is carried in a glass tube securely affixed to the top of the case, and provided

above with a divided rotating head. This tube was shielded with an outer jacket of velvet folded several times around it. The entire apparatus was fitted up in the cellars of the Geological Laboratory, Trinity College, being fixed to the massive basement-wall of the building by a stout shelf; the temperature in this cellar varied slowly, seldom as much as one degree in twenty-four hours during the time of the experiments. The distance of the scale on which the spot of light was received was such that the translatory motion of the vessels containing the active substances was magnified just eleven times. The divisions on the scale used are arbitrary; and a single division is nearly three-fifths of a millimetre. Thus a motion of ten divisions of the spot of light on the scale corresponds to a rectilinear displacement of the vessels amounting to a little more than half a millimetre. The plane of the beam is due north and south.

In spite of all endeavours to attain freedom from inequalities of temperature within the apparatus, the chief difficulty encountered in making the experiments has been with draughts within the case. This will readily be understood when the very great bulk which it is necessary to confer upon the load carried at one extremity of the beam is taken into account. Not only have bulky vessels to be there suspended, but an insulating jacket has further to be provided. Thus, in the experiments with which up to the present I have been principally occupied—those on the solution of copper sulphate in acid water—the paper cylinder within which the whole is contained measures 7 cms. in diameter by 8 cms. in length. Such a large bulk is so effectively acted upon by convection-currents within the case that it was, in truth, found to be never at rest, but to be continually experiencing slow but persistent displacements.

Attempts to determine a consistent logarithmic decrement were not very satisfactory, the damping being not only rapid but occasionally irregular. The determination of the sensitiveness of the loaded beam to a small force applied to one extremity of it by a direct method was fairly successfully accomplished, as will presently be seen. But first the influence of the suspension as a constraint on a displacement from a zero position must be considered.

The time of vibration of the fully loaded beam was found to be just thirteen minutes. This was with a load of 80 grammes at either end of the beam, the quantities of chemicals used being about one-fourth of those used in Heydweiller's experiments, as will be seen later. Taking the radius of gyration of the load as 12·5 cms., the force required to be applied to one extremity of the beam in order to confer a twist of one degree on the suspension calculates out as 0·00226 dynes. The twist is not a pure torque. The effect of this torque will be to displace the spot of light 2·54 cms. on the screen, or just forty divisions of the scale used. This corresponds to a linear displacement of the vessels through a distance of a little over 2 mms. The effects of so feeble a restraint acting at forty divisions of

the scale at either side of zero may be left out of account as not actually influential in restricting the sensitiveness of the apparatus.

To investigate the more serious question of the effects of air-friction, a fine needle was magnetised, and then broken in halves. One half was suspended within a tall glazed box by a fibre of silk 54 cms. in length. The needle was balanced horizontally, and, of course, pointed in the magnet meridian. The other half of the needle was now brought to within 3 cms. of the suspended needle, attracting it out of the vertical. A micrometer microscope served to measure the displacement of the fibre just above the needle. It was found to be 0·09 cms. The weight of the suspended needle was 0·0230 grms. The force at 3 cms., therefore, is 0·039 dynes. One of the magnets is now attached to the extremity of the beam, and at right angles to the plane of the beam, and is thus nearly perpendicular to the magnetic meridian. The other magnet is mounted at the end of a wooden rod, and introduced through the wall of the case containing the beam. To avoid any air-disturbance attending this operation, the rod is arranged to move within a glass tube entering the case from outside, and closed at its inner end. Thus the magnet can be presented, at any desired distance, to the magnet carried on the beam. It is now sought to determine the rate of change of velocity of the spot of light when the magnet is presented at various distances. Many observations were made. These were examined by plotting the rate of motion before and after the introduction of the magnet from without. I reproduce further on (see p. 38) some of these observations as typical of the results. The mode of observation is the same here as was adopted in the actual experiments on possible inertia change—that is, the position of the spot of light on the divided scale is read every 15 seconds. The scale reads 0 at its central point, and, as already stated, is divided to each side of 0 into divisions three-fifths of a millimetre in length. The scale-readings are plotted vertically, the time-intervals horizontally, the units in the latter case being 15 seconds. Thus the intersection of the curve with each ordinate is an experimentally determined point.

It will be seen that for several consecutive intervals of 15 seconds, the beam had fluctuated irregularly, suffering minute displacements. When now the magnet is introduced, a steady motion is imparted to the beam (which is fully loaded), attaining at the moment of the second observation after application of the force—that is, in 30 seconds—a velocity which is almost steady, and amounts, in the mean, to the rate of 15 scale-divisions per 15 seconds, or one scale-division per second. The magnet being presented at an initial distance of 3 cms., we may state this result as indicating that a force of 0·0392 dynes acting for 30 seconds imparts a velocity of one scale-division to the spot of light, or 0·055 mms. per second to the masses suspended on the beam. Now this total mass being here just 160 grammes, it is easy to calculate that a force of 0·0288 dynes acting for this interval of time

upon such a mass should in the absence of all frictional or other restraints produce the observed velocity. It follows that the force $0{\cdot}0392 - 0{\cdot}0288 = 0{\cdot}0104$ dynes is expended in doing work in overcoming frictional resistance, and is doubtless, in small part, stored up in elastic potential in the suspending wire. This experiment is very instructive as to the sensibility of the arrangement. It may safely be said that the effect of the force of just 0·04 dynes for the interval of 30 seconds is perfectly distinctive, and could not be mistaken for any of the usual perturbations to which the beam is subject, arising from air-convection. Undoubtedly a still less force acting for this period would be distinctive; but as this force has been directly shown to be so in repeated experiments with the magnets, it is more satisfactory to accept this as the sensibility of the apparatus, just as we define the sensibility of a balance by a fraction above that which can be attained. We assume, then, that a force of 0·04 dynes acting for 30 seconds cannot in a series of experiments be confounded with the perturbation to which the measurements are subject.

It remains to estimate what fraction of the force that might be expected this distinctive force represents. The dimensions upon which the more recent experiments have been carried on may be exemplified by the experiments on the solution of copper sulphate in water acidulated with sulphuric acid. Now Heydweiller's quantities (*loc. cit.*, p. 410) are about 50 grms. of the salt, 150 grms. of water, and 7 grms. of sulphuric acid. This will leave a residue of undissolved salt. Hence the quantity of liquid defines the amount of the reaction. I take from 25 to 30 grms. of the salt, and the same weight of water which has been acidulated in the proportions adopted by Heydweiller. The object of varying the proportions of solid and liquid is to secure the saturation of the latter under conditions where stirring cannot be resorted to, and rapid solution is essential. In fact, an excess of copper sulphate is used solely in order to provide increased surface. In the proportions used, the solid being brought to a very fine powder by prolonged grinding in an agate mortar, the liquid on its descent to the lower vessel is, as it were, entirely immersed, or nearly so, in the salt, if the expression is permissible. The rate of solution may be assumed to be almost as fast as the commingling of liquid and solid. Now it was found that it required from 25 to 30 seconds for the transport of the liquid from the upper vessel to the lower. We take it, therefore, that in 30 seconds 25 grammes of the acidulated water becomes saturated. This is just one-sixth of the quantity used by Heydweiller. Now it was calculated above that, assuming Heydweiller's weight-loss as 0·1 mgr. in 200 grammes, and that this is effected in 60 seconds, a force of 2·54 dynes would act according to the equation for the change of velocity:

$$V - v = \frac{mv}{2M}$$

In the experiments I am describing, the value of m would be one-sixth of Heydweiller's result, and obtained over the interval of 30 seconds; and Heydweiller's result indicated a weight-loss of 0·08 milligrammes in 200 grammes. The total mass is now 160 grammes. The value of $V - v$ becomes, therefore,

$$\frac{0{\cdot}00008 \times 3 \times 10^6}{6 \times 30 \times 2 \times 160} = 4 \times 10^{-3} \text{ cms. per second};$$

and the force causing such acceleration would be 0·640 dynes. It would appear, therefore, that the sensibility of the apparatus (0·04 dynes) is such as to show the one-sixteenth part of the force calculated from the equation and Heydweiller's results. In short, the method is sensitive to about 6 per cent. of the calculated amounts. This result will serve as typical of the dimensions of the experiments presently to be given in detail. Owing to small changes made in the quantities employed and mass of containing vessels, &c., the sensibility will vary a little about this amount; but calculation in each case would serve no purpose.

It remains to consider if the experiment is not open to errors of a magnitude comparable with the small effects being dealt with.

The most obvious source of error will reside in temperature changes accompanying the reactions progressing in the closed vessels. Forces arising from this source will be irregular or indeterminate in their effects; that is, if the beam is symmetrically placed in the containing case, it will be impossible to predict the direction in which draughts arising from escaping heat will urge the beam. If the vessels are nearer one side of the case than another, there would probably be effects resembling attraction towards the nearer wall. The only way, of course, to deal with such source of error is to seek to eliminate it altogether for an interval of time sufficient to permit of the observation of any force arising from the reactions. So far as the experiments on copper sulphate are concerned, the jacket of about one centimetre thickness of dry cork-dust surrounding the vessels appears to sufficiently accomplish this object. The fall in temperature attending the solution was, in fact, found to be only a little above one degree Centigrade; and that this small rise could produce an effect across the jacket in the space of two or three minutes is not to be anticipated. However, direct experiments, to be given later, appear to negative any error from this source arising during the first three minutes.

Another and more instantaneous effect will arise out of a change of temperature of the air around the vessel. Its contraction or expansion, as the case may be, will lead to a small inflow or outflow of air at the mouth of the cylinder. When it is considered, however, that only the small volume of air in the interstices of the insulator is concerned, and that only a part of this changes its volume by a little more than one-third per cent., and further, that this flow is distributed over

a large area, the surface of which, so far as it lacks symmetry, is varying with each experiment, the idea of a directed acceleration arising from this source must, I think, be excluded. What error there is, supposing it appreciable, which is very doubtful, must be irregular in its effects.

Again there is nothing in the hanging of the paper cylinder containing the vessels to determine in one direction more than in another any resultant force arising from the effects of temperature; we therefore do not expect that in successive experiments it will take the same direction, and change according as the observations are effected at noon or midnight.

No error from the momentary heating of the radiator when melting the plug could be detected, although repeated trials were made. One of these is given in detail further on. The isolation of the radiator in the cell was, however, found by preliminary experiments to be essential to its successful working.

It is probable that there is some adjustment of the point of centre of gravity of the vessels during the thirty seconds occupied by the liquid in descending to the lower vessel. This will doubtless give rise to a corresponding shift of the beam about its centre of suspension. But I have not been able to detect any acceleration of the masses arising from this source. Similarly the kinetic effect of the falling liquid is balanced by internal reactions, leaving no external resultant. A slight tremor or vibration of the light-spot may sometimes be observed to attend the descent of the liquid. These mechanical effects are again not a source of trouble so far as I have observed. Like thermal effects, they would, if existent, be indefinite as regards direction. In the earlier days of the experiments, not being quite satisfied that residual effects from such sources might not exist, I made experiments in which the conditions were specially arranged to favour the development of mechanical effects, the vessels being hung at a considerable angle out of the vertical. I could not detect the smallest effect. In these experiments, the upper vessel was filled with 30 grammes of distilled water, and the lower one with the same weight of sand.

There remains still a mechanical effect of assured existence and of directed nature—that arising from the fact that the liquid leaving the upper vessel possesses a velocity greater by a very little than the lower vessel into which it falls, being further from the centre of the earth. As might be at once anticipated, the effect is very small, and, in fact, outside the limits of present observation. Assuming that one gramme of liquid falls per second through the height of 2 cms., and taking the radius of the Earth as 64×10^7 cms., and the diurnal velocity as 3×10^4 cms. per second, we find that the excess velocity which the liquid possesses amounts to 10^{-4} cms. per second. A force amounting to about the ten-thousandth part of a dyne acts on the vessel. This force would not be determinable with the present arrangements. It will always act in the one direction—that is, from west to east.

As before stated, the chief source of annoyance is the want of thermal equilibrium of the air within the case. I hope to be able to reduce the effects of this in future experiments, not only by carrying out the experiments on a smaller scale, but by removing a large part of the air from around the beam and its load, using an air-tight vessel of much smaller size than the present one. Although not hoping to carry the vacuum so far as to effect the viscosity of the contained air, a reduction of its mass, and corresponding reduction of kinetic effects arising from draughts, would, I anticipate, greatly improve the conditions of experiment.

It was at first intended to study the effects, if any, of the reactions by the perturbations of the curve of vibrations as determined by the motion of the light-spot. It was soon found—as already stated—that when the vibrations got sufficiently small in amplitude to give the requisite sensibility, they were subject to irregularities which rendered any reliance on the form of the curve unsatisfactory. In fact, the vibrations have become aperiodic before the conditions suitable to experiment are attained. Frequently, as long as a couple of hours of patient observation had to be put in before a motion sufficiently slow, and at the same time sufficiently regular, was reached. It is very questionable if so feeble a constraint from the suspension was altogether desirable. However, it is to be remarked that a suspension sufficiently rigid to control the effects arising from draughts would itself prove a serious restraint on the motion of the beam. I therefore resolved to try and seize favourable moments when a slow and apparently steady motion was in progress, and then seek to determine if the effect of the experiment was to produce any perturbation on this motion. While I believe I have in this way probably got as much out of the apparatus as is possible, a study of the results which follow will show how unsatisfactory it is when very small perturbations come to be sought for. At the instant of experiment, we are, in fact, not aware of the tendency of the motion beyond a short distance. The torque of the suspension may be dismissed. It will not act as a sudden interruption to the steadiness of the motion. The difficulty is with air-currents within the case. All that can be done is to compare the curves of experiment with those obtained under precisely similar conditions, when a known force was applied to the load. It is evident that a change of velocity so great as that obtained under the force of 0·04 dynes, as before referred to, would be conspicuous among the perturbations affecting the motion of the beam before and during the experiments.

In interpreting the tables and curves which refer to the inertia experiments, it has to be borne in mind that the vessels are hung on the south extremity of the beam. The scale receiving the light-spot lies to the west side of the beam; and the spot is at 0 when the beam is due north and south. The scale being divided and numbered to either side of 0, it is necessary to distinguish these numbers in order to know where on the scale the spot stands at any reading.

The scale being also north and south, the south end of the scale is distinguished by numbers in italics, the north end by ordinary type. Thus if the numbers in italics are diminishing, the light-spot is moving from the south end of the scale towards the centre, and consequently the vessels are travelling westward. The same direction of motion prevails if the numbers in ordinary type are increasing. On the other hand, the motion of the vessels is easterly if the ordinary numbers are diminishing, the figures in italics increasing. The results are plotted so far as is consistent with reasonable economy of space. In the curves *upwards* is equivalent to easterly, and *downwards* to westerly motion of the vessels. The full horizontal line indicates the zero of the scale, and from this the scale divisions are laid off vertically, 20 scale-divisions to 1 centimetre.

The intersection of the curve with each ordinate is a point fixed by a reading of the position of the light-spot on the scale. The ordinates are uniformly fifteen seconds of time apart, the observations being made at this time-interval.

EXPERIMENTS.

Experiment on the Conductivity of Heat through the Jacket of Cork-dust.

A glass bulb of about the size and thinness of the lower bulb used in the experiments, containing 27 ccs. of water, at a temperature of 16·60, and closed by a cork through which passes the stem of a sensitive thermometer, is placed in the paper cylinder, and surrounded by the jacket of cork-dust just as in the experiments to be described. The whole occupies the usual position in the zinc-lined box. A second thermometer placed in the box gives the temperature of the air within the box. The times of observation beginning at 0 minutes, and the thermometer readings corresponding in the cooling water T, and in the air t, were observed as follows:—

0	minutes	T 16°·60, t 12°·80.
40	,,	T 15°·35, t 12°·65.
65	,,	T 14°·80, t 12°·69.
90	,,	T 14°·40, t 12°·68.
115	,,	T 14°·10, t 12°·62.
140	,,	T 13°·80, t 12°·57.
165	,,	T 13°·58, t 12°·54.
195	,,	T 13°·32, t 12°·50.
220	,,	T 13°·19, t 12°·48.

If these results are plotted, it will be found that when the steady state is reached, and with a difference of temperature of 1·2 degrees, the fall is at the rate of one degree in 11·7 minutes. Taking the water equivalent of the glass bulb (15 grms.), and of the thermometer bulb together as 3 grms., we find that 30 calories pass

through in the 115 minutes, or 0·0043 calories per second. This result is recorded as giving some idea of the value of the jacket as a non-conductor of heat; but it does not directly answer the question as to the interval elapsing after the mixing of the substances during which the alteration of temperature arising in the solution will not have sensibly altered the external temperature of the paper cylinder.

Experiments on the rate of Propagation of a temperature change through the Cork-dust Jacket.

One of the glass vessels used in the experiments presently to be given is charged with 25 grms. of CuSO in the lower bulb, and with 25 ccs. of water in the upper bulb, and arranged within the cork-dust jacket and paper cylinder precisely as in the experiment. The weight of the cork-dust + paper cylinder is 16·3 grms. A second cylinder of cardboard surrounds the paper cylinder with an annular space of about 1 cm. intervening. In this annular space a sensitive thermometer is placed, and the upper opening of the annular space closed with cotton wool.

The whole is placed in the zinc-lined box in the cells, the tube containing the fusible plug, and which controls the descent of the water, extending into the glazed cell in front of the box and above the platinum heater. Through this window the thermometer is observed by a lens. Twenty-four hours after setting up, the heater is used to melt the plug, and the observations of temperature made.

Before contact the thermometer stood at 11°,03 steadily. For the first complete four minutes no change was detected. For the next two minutes there was uncertainty as to whether any movement was taking place, the temperature was at last entered as 11°.025 as attained at end of the sixth minute and the movement beginning during the fifth minute. During the seventh minute the thermometer fell to 11°·015; in the eighth minute to 11°·01; in the ninth, 11°·00. The experiment was then dismantled, and the thermometer inserted in the solution, when it read 9°·9. This was thirteen minutes after commencement.

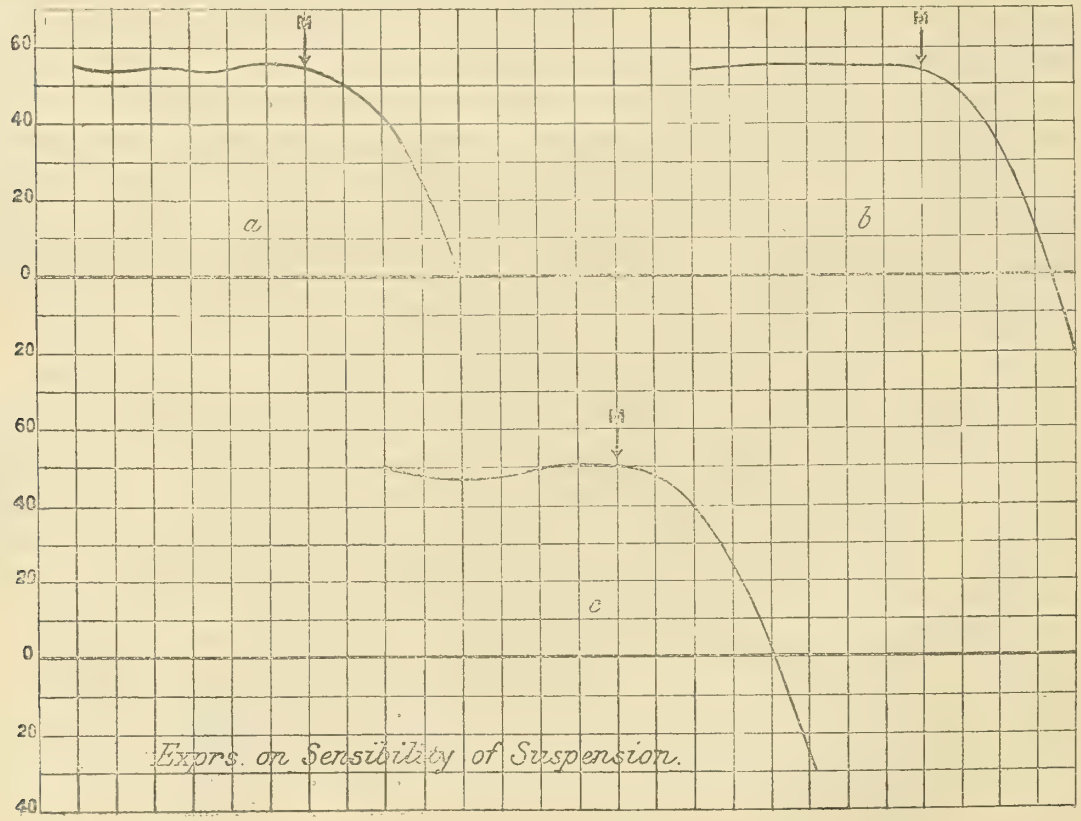
A second experiment, using a weight of cork-dust just $3\frac{1}{2}$ grammes greater than in the last experiment, 20 grammes of $CuSO_4$ and 20 ccs. of water, gave the following results:—The initial temperature in the annular space was 10°·95, steady as observed for ten minutes. After contact no change whatever was perceived for six minutes. In the course of the seventh minute, a fall of 0°·01 was determined by aid of the lens. Finally the complete experiment stood as follows:—

Initial temperature, ...	10°·95
In $6\frac{1}{2}$ minutes, ...	10°·94
In 7 ,, ...	10°·93
In 8 ,, ...	10°·92
In $8\frac{1}{2}$,, ...	10°·91
In $9\frac{1}{2}$,, ...	10°·90

The thermometer was now inserted into the solution, when it fell to 9°·8. Hence the fall in the solution after about ten minutes was 1°·1.

From these experiments, it would appear that the air around the paper cylinder experiences a minute increase of density due to the first absorption of heat by the solution beginning probably during the fourth minute (or near the end of it) from the moment of contact. I may observe that I can detect nothing in the arrangement of the experiment to lead to the inference that a minute change of density of the air in contact with the paper vessel could lead to a horizontal force directed in any particular direction. Although we cannot suppose the cylinder to present perfectly perpendicular sides, what orientation to the vertical obtains must continually vary with each fresh arrangement of the vessels within it and instalment of it in position.

Experiments on the sensibility of the apparatus.—Three experiments, using a magnet to accelerate the loaded beam. At the instant marked M, the magnet is applied (see fig. 1). The scale-readings before and after the force is applied show that an approximately steady state was attained at the moment of making the experiment, and that an acceleration resulted causing a rapidly increasing movement of the light-spot. The mean numerical value of the acceleration after the force has acted for thirty seconds is already given. It is derived as follows from the observations.

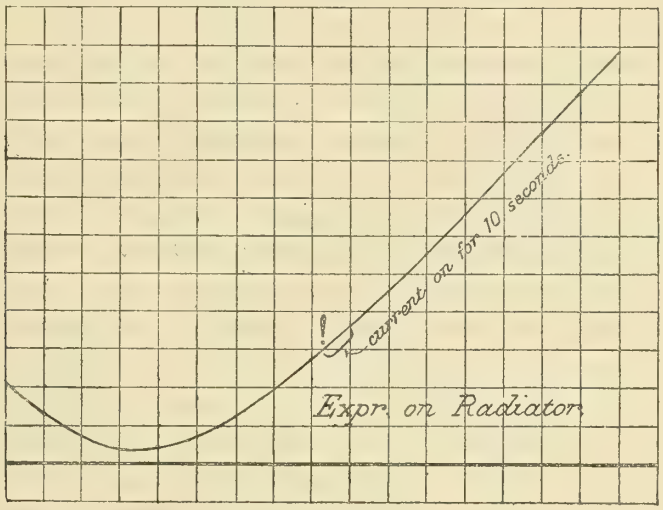


The result of experiment *a* is a velocity conferred at end of thirty seconds from moment of application of magnet amounting to fifteen scale-divisions per fifteen seconds. The result of experiment *b* is a velocity at end of thirty seconds of eighteen divisions per fifteen seconds. Experiment *c* gives at end of thirty seconds a velocity of 12·5 divisions per fifteen seconds. The mean is 14·5 divisions per fifteen seconds, or nearly one division per second.

Experiment on the effect of liberating the liquid in the upper vessel by the melting of the fusible plug.

The experiment (one of many similar) was made on January 14th. Water was placed in upper vessel and sand in lower vessel. Hour, 4.30 p.m. It will be seen (fig. 2) that the curve is undisturbed by the application of heat to the connecting tube, and that the descent of the liquid apparently produces no perturbation.

Readings obtained at intervals of 15 seconds:—*21, 13, 7, 4, 4, 7, 12, 18, 27,* !, *36, 56, 65, 76, 87, 97, 107, 116.* The sign ! marks the instant in which the contact was depressed for the couple of seconds required to melt the plug.

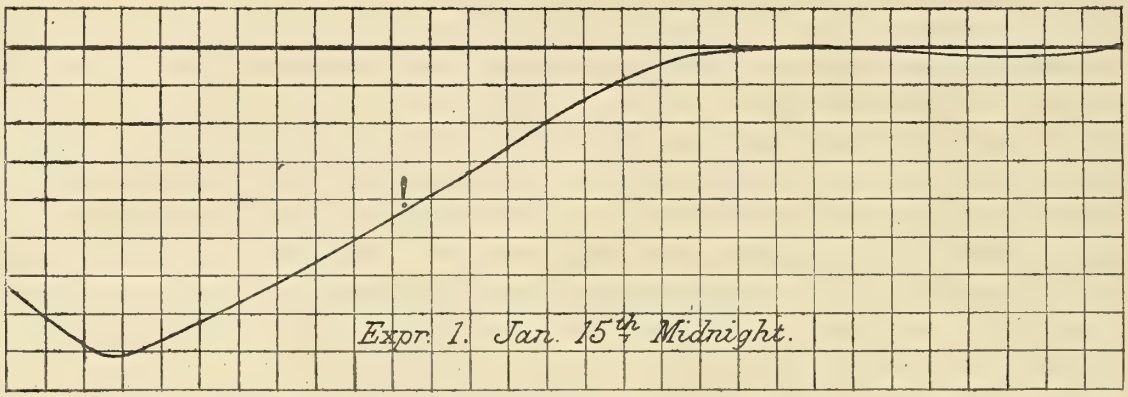


In the following experiments, the mass of copper sulphate varied from 20 to 25 grammes. In the earlier experiments the solvent is very dilute sulphuric acid, in later ones water only; the quantities being about 26 c.c. where the acid was used, and 25 when water only was used. The weight of the glass vessel was in earlier results nearly 12 grammes; later one of 13·7 grammes, and finally one of 15·7 grammes had to be used, owing to accidents. The weight of cork-dust jacket and paper cylinder varied at different times from 16·3 to 22·5 grammes, the higher values being adopted in the later experiments. The total mass varied between 80 and 85 grammes in round numbers.

Experiments on the solution of Copper Sulphate in dilute Sulphuric Acid.

(1). *Experiment made at midnight, Jan. 15th.*—$CuSO_4$, 25 grms.; H_2O 25, c.c.; H_2SO_4, 1·8 grms.

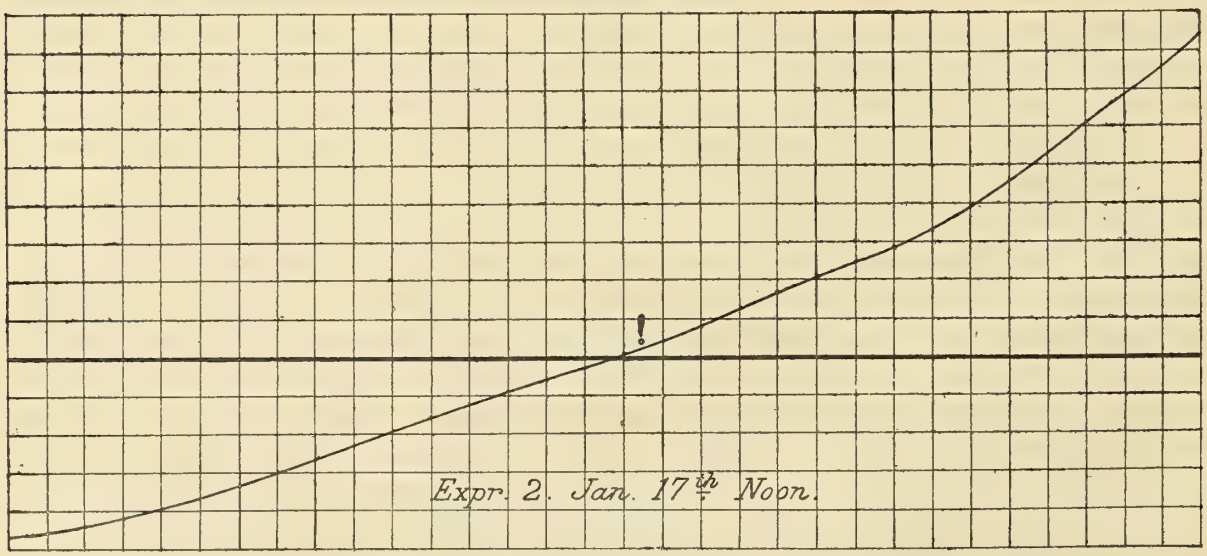
Readings: 62, 71, 78, 81, 83, 81, 77, 72, 67, 62, 57, 50, 45, !, 40, 34, 27, 20, 15, 9, 4, 3, 1, 0, 0, 0, 1, 2, 3, 3, 3, 2, 0.



The load is travelling with almost uniform velocity in an easterly direction at the moment of the experiment. There is an almost inappreciable acceleration in this direction, which, one and a half minutes after the contact, is rapidly checked. A normal vibration would involve now that the beam should swing westerly, but it maintains its deflection. A possible interpretation of this would be that an easterly force retarded the return-vibration.

(2). *Jan. 17th, noon.*—$CuSO_4$, 25 grms.; H_2SO_4, 1·8 grms.; H_2O, 35 c.c.

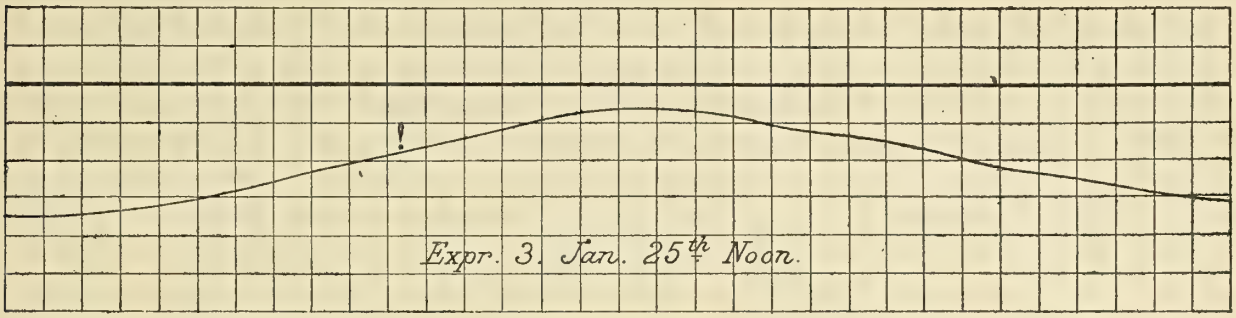
Readings: 47, 46, 44, 42, 39, 37, 33, 30, 27, 23, 19, 16, 13, 9, 6, 3, *1*, !, *4*, *8*, *13*, *17*, *21*, *24*, *28*, *33*, *39*, *46*, *52*, *61*, *68*, *75*, *84*.



There is no indication of westerly acceleration in this result. What change attends the experiment is rather a very feeble increase of velocity eastwards.

(3). *Jan. 25th, noon.*—$CuSO_4$, 20 grms.; $H_2O + H_2SO_4$, 24 c.c.

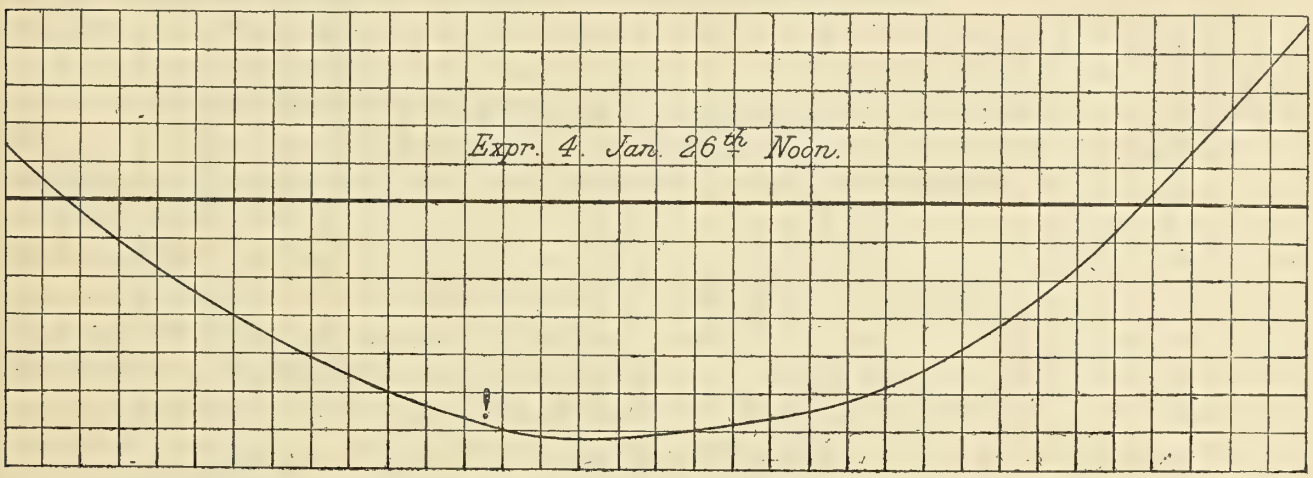
Readings: 34, 34, 34, 33, 32, 30, 28, 26, 23, 21, 19, !, 14, 12, 9, 7, 7, 7, 7, 9, 11, 13, 14, 15, 17, 20, 22, 24, 25, 27, 30, 32.



Found by trial that the quantity 24 c.c. of liquid took just 60 seconds to flow from above into the lower vessel. It is evident, therefore, from the curve, that it was just as the transfer of the liquid was completed that the easterly motion of the load turned to a westerly motion, which continued during the time of observation.

(4). *Jan 26th, noon.*—$CuSO_4$, 20 grms.; $H_2O + H_2SO_4$, 25 c.c.

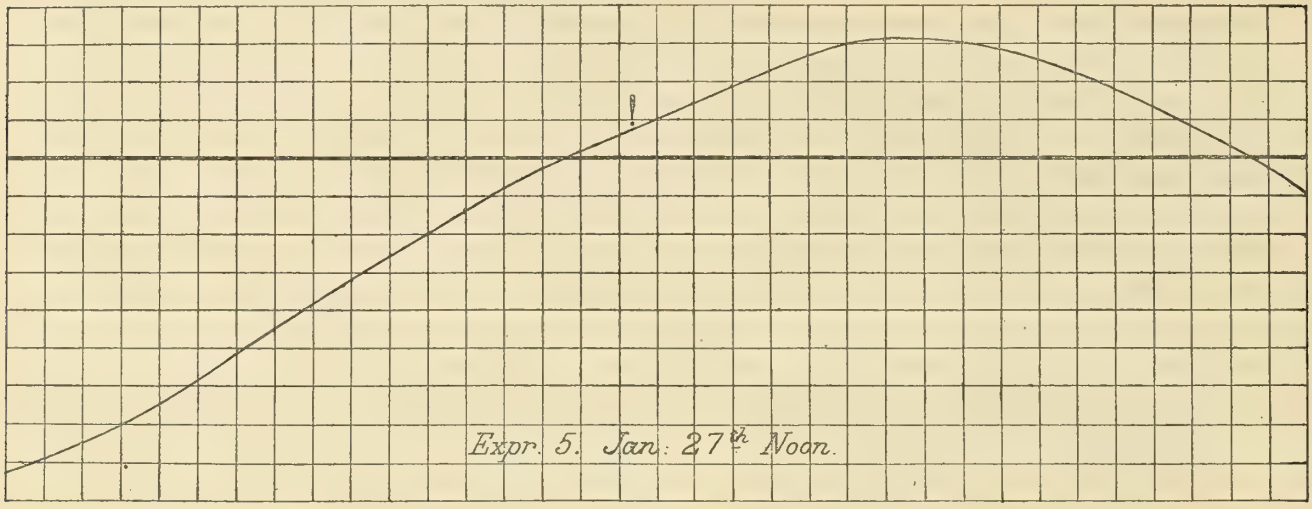
Readings: *16, 6,* 3, 10, 24, 30, 36, 41, 45, 50, 53, 57, !, 59, 62, 62, 62, 61, 60, 58, 57, 55, 52, 48, 44, 39, 32, 25, 17, 7, *3, 14, 24, 36, 48, 50.*



Close examination of the curve shows that the swing to westward which was coming to an end at the moment of the experiment, has apparently been rather abruptly terminated, and a period of delay substituted retaining the load at its westerly deflection before the return swing eastward properly begins. The interference with the normal vibration is only such as would arise from a very feeble force restraining the motion of the beam during the fall of the liquid, and for some sixty seconds after. A preponderating direction can hardly be ascribed to the force.

(5). *Jan. 27th, noon.*—$CuSO_4$, 25 grms.; $H_2O + H_2SO_4$, 25 c.c.

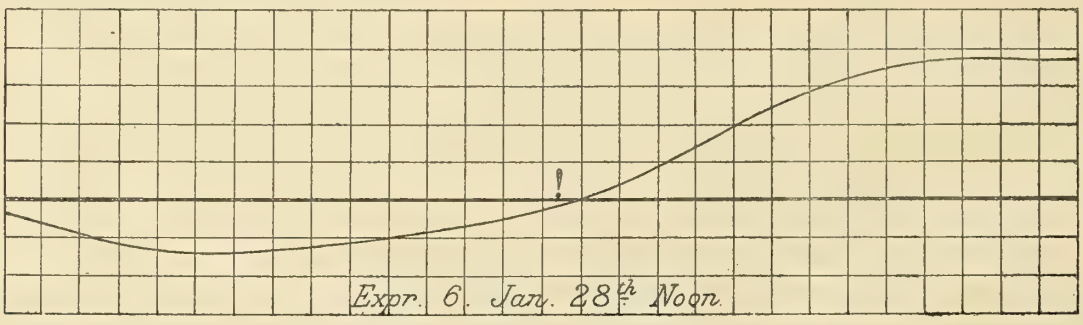
Readings: 45, 55, 65, 73, 80, 86, 90, 92, 93, 92, 90, 86, 83, 78, 74, 70, 63, 57, 51, 44, 38, 32, 25, 19, 13, 8, 3, *1*, *5*, !, *9*, *14*, *18*, *22*, *27*, *29*, *30*, *30*, *29*, *28*, *25*, *22*, *18*, *13*, *8*, *3*, 2, 9, 17, 23, 32, 41, 50.



About 30 seconds after the complete descent of the liquid, the curve shows that the slow easterly motion of the beam ceased, and a westerly drift began. A force acting in a westerly direction, and generated during the solution, would occasion such an appearance.

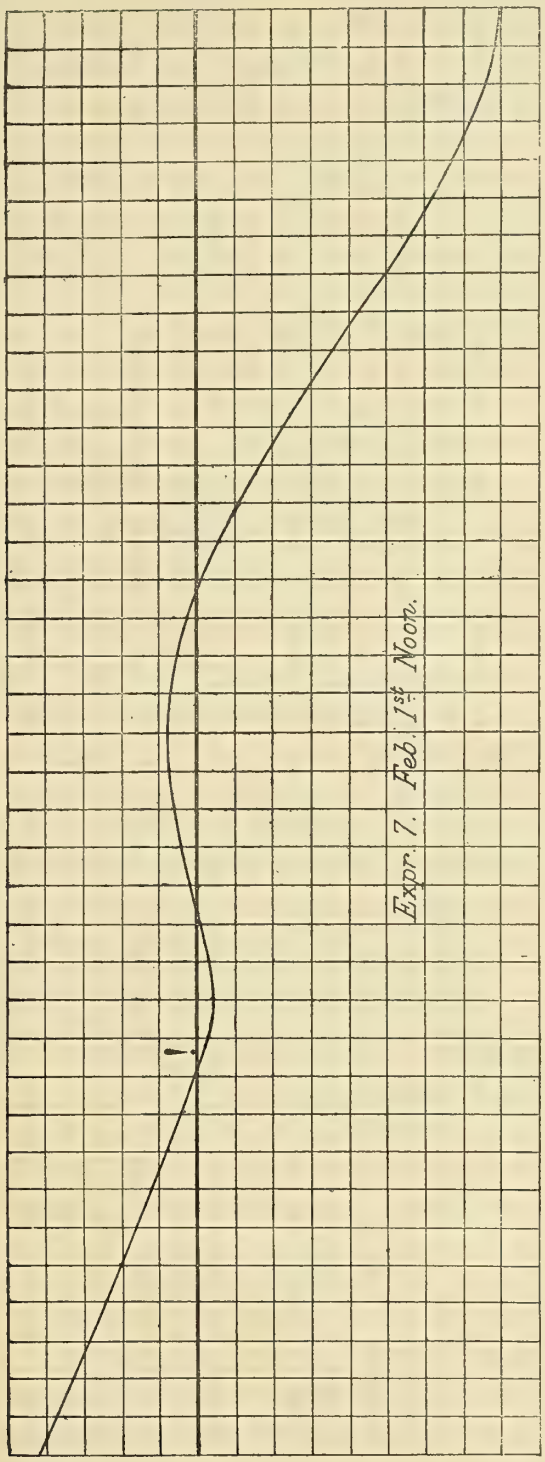
(6). *Jan. 28th, noon.*—$CuSO_4$, 25 grms.; $HO + H_2SO_4$, 25 c.c.

Readings: 3, 5, 9, 11, 13, 14, 14, 13, 12, 11, 10, 9, 7, 5, 2, !, 0, *4*, *9*, *15*, *20*, *24*, *28*, *32*, *34*, *35*, *37*, *37*, *37*, *37*.



Called away at this point, and on return in fifteen minutes, I found that the load had continued its motion westward, and the beam was now at rest against the westerly stop. A very slight flattening of the curve towards the west is perceivable after the complete descent of the acidulated water; finally, the motion is westerly.

(7). *Feb.* 1*st*, *noon.*—$CuSO_4$, 20 grms.; $H_2SO_4 + H_2O$, 20 c.c.

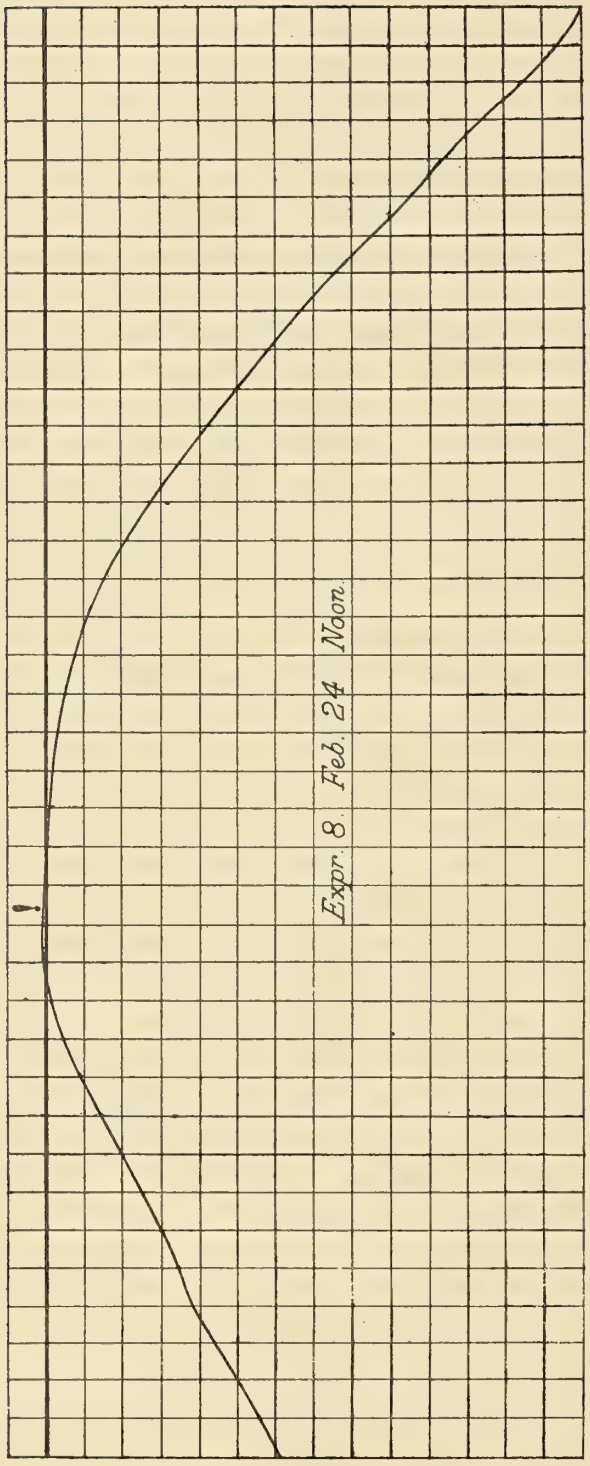


Readings: *50*, *56*, *42*, *38*, *33*, *29*, *25*, *20*, *15*, *12*, *7*, *3*, 0, !, 3, 5, 3, 1, *1*, *3*, *6*, *7*, *7*, *6*, *5*, *2*, 1, 6, 11, 17, 22, 29, 46, 43, 50, 55, 61, 67, 71, 75, 78, 80.

In this experiment a new glass vessel was in use, the one used in the former experiments having been broken. It was found by trial that 55 c.c. take only 30 seconds to fall from upper to lower compartments. The curve shows that a marked perturbation occurred at the time of descent of the liquid, the westerly motion being changed to easterly; but this again changes to a westerly, which, at a somewhat faster rate than the original westerly motion, persists for five minutes after the mixing of the substances. There is a possibility that the easterly bend was not introduced by any effect arising directly from the experiment. If so, its brief persistence would point to a westerly force arising on the solution.

Experiments on the Solution of Copper Sulphate in Water.

(8). *Feb. 24th, noon.*—$CuSO_4$, 25 grms.; H_2O, 25 c.c.

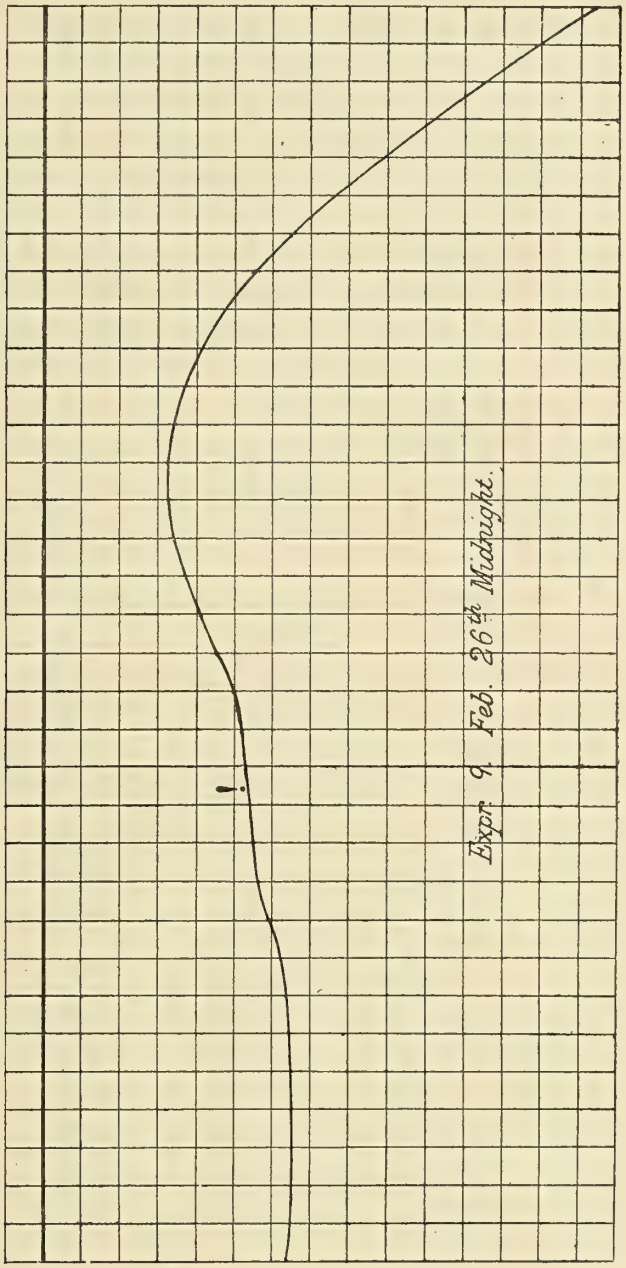


Readings: 44, 55, 64, 72, 78, 83, 86, 88, 90, 90, 89, 87, 84, 82, 78, 74, 69, 65, 60, 55, 50, 44, 38, 35, 38, 25, 20, 14, 10, 4, 1, 0, 0, !, 0, 1, 1, 2, 3, 5, 8, 11, 16, 22, 27, 35, 43, 51, 58, 67, 76, 87, 95, 104, 115, 125, 134, 140. Stop.

The curve shows a westerly acceleration after experiment, causing an elongation westward greater than the extreme point attained in the previous vibration. There was a small tremor visible during the time of descent of the water.

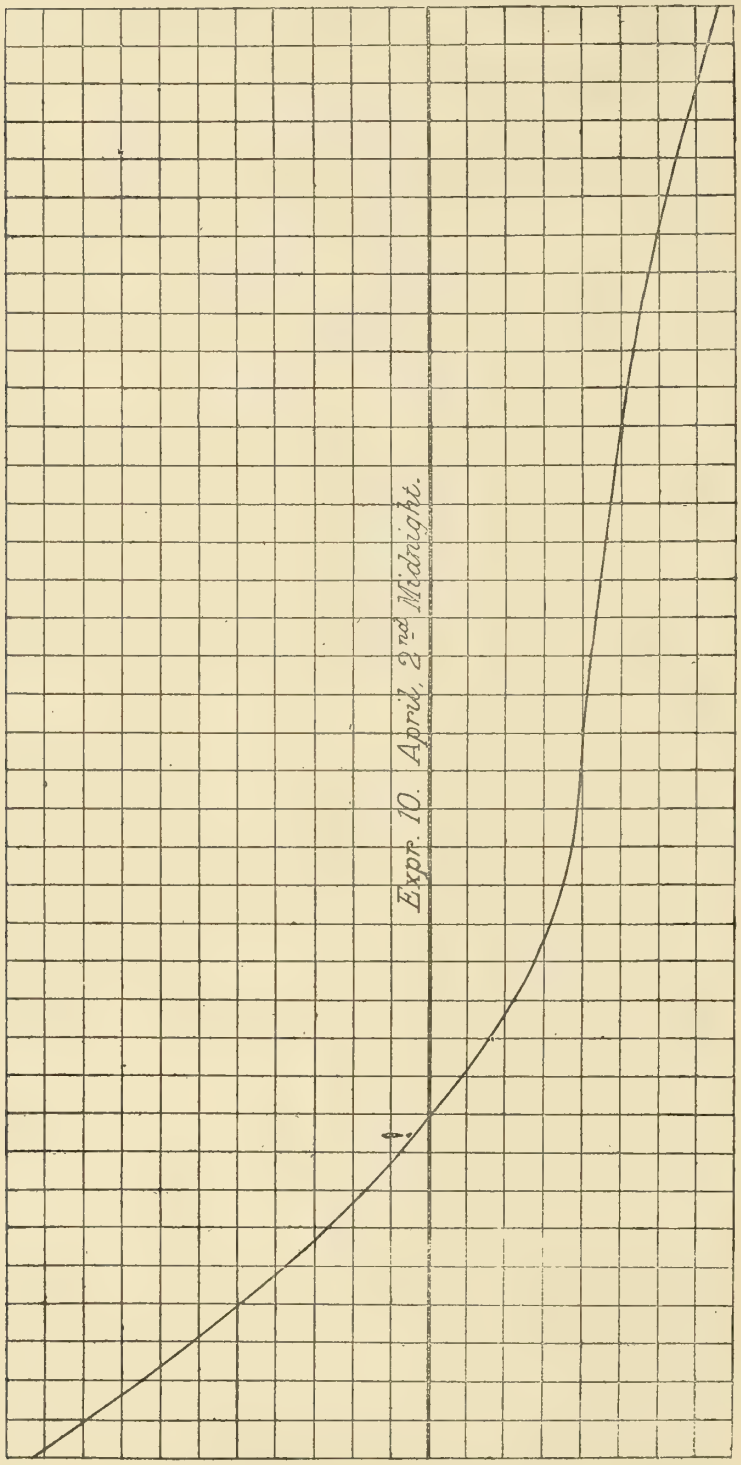
(9). *Feb. 26th, midnight.*—$CuSO_4$, 25 grms.; H_2O, 25 c.c.

Readings: 60, 61, 62, 63, 63, 63, 64, 65, 65, 65, 65, 65, 64, 63, 62, 59, 56, 55, 54, !, 53, 52, 50, 45, 41, 38, 35, 33, 33, 35, 37, 42, 48, 57, 65, 77, 89, 102, 115, 130, 145.



The night was gusty and wild. For one and a half minutes after complete descent of water the easterly drift is accelerated; then a rapid motion to westward. In reference to this experiment, it is to be observed that in gusty weather unsteady motions of the beam were frequently observed.

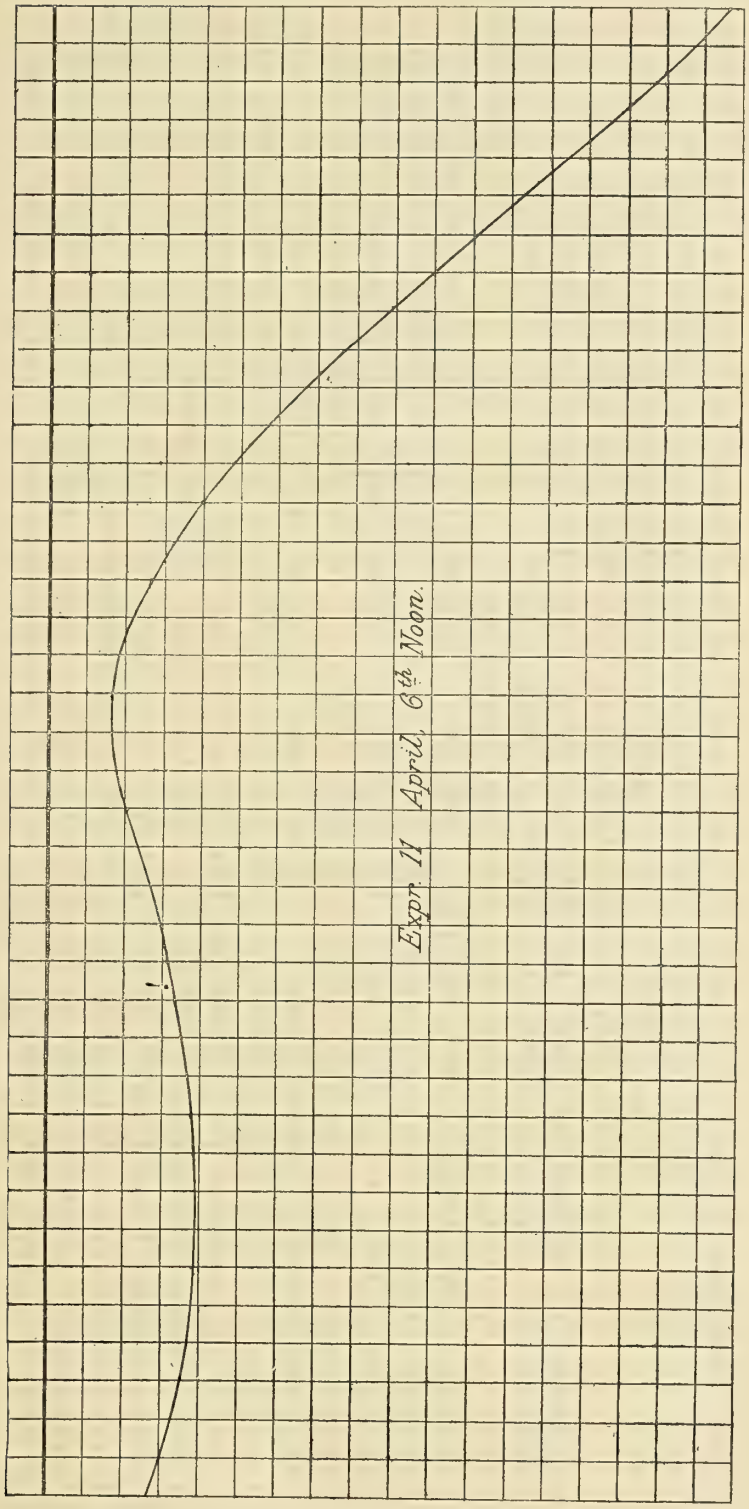
(10). *April* 2, *midnight.*—$CuSO_4$, 25 grms.; H_2O, 25 c.c.



Readings:—*103*, *90*, *75*, *62*, *50*, *37*, *26*, *16*, *8*, !, 1, 10, 16, 22, 27, 32, 35, 37, 39, 40, 41, 42, 43, 44, 46, 47, 48, 50, 51, 52, 54, 56, 58, 60, 63, 66, 68, 71, 74, 76, 79, 82, 84, 86, 87, 90, 90, 92.

The curve might be interpreted as showing the action of an easterly force damping the motion westward which obtained at the moment of making the experiment. It is also readable as indicating a restraining westerly force retaining the load at its westerly elongation. The former seems the more likely reading, regard being had to the latter part of the curve.

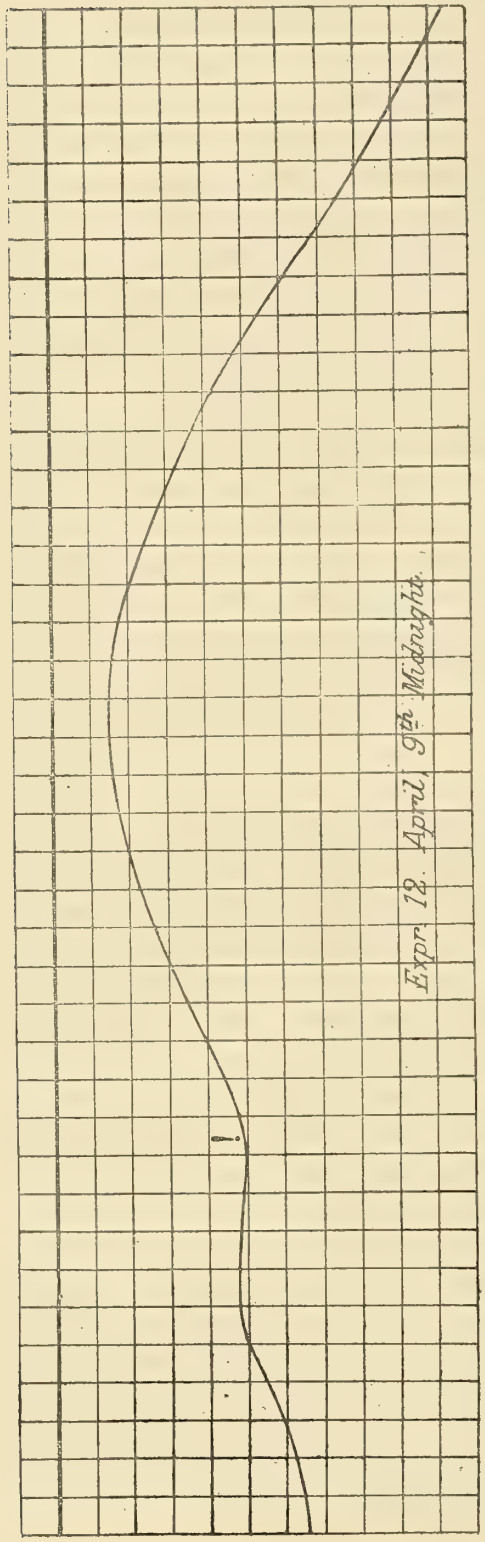
(11). *April 6th, noon.*—$CuSO_4$, 20 grms.; H_2O, 25 c.c.



Readings: *4*, 3, 6, 11, 16, 20, 25, 27, 30, 33, 35, 37, 38, 39, 39, 39, 39, 38, 37, 36, 34, !, 32, 30, 26, 24, 20, 18, 17, 17, 19, 22, 27, 33, 40, 48, 57, 67, 78, 89, 100, 111, 122, 134, 145, 156, 167, 177. Stop.

The experiment is, on the whole, apparently indicative of a westerly force.

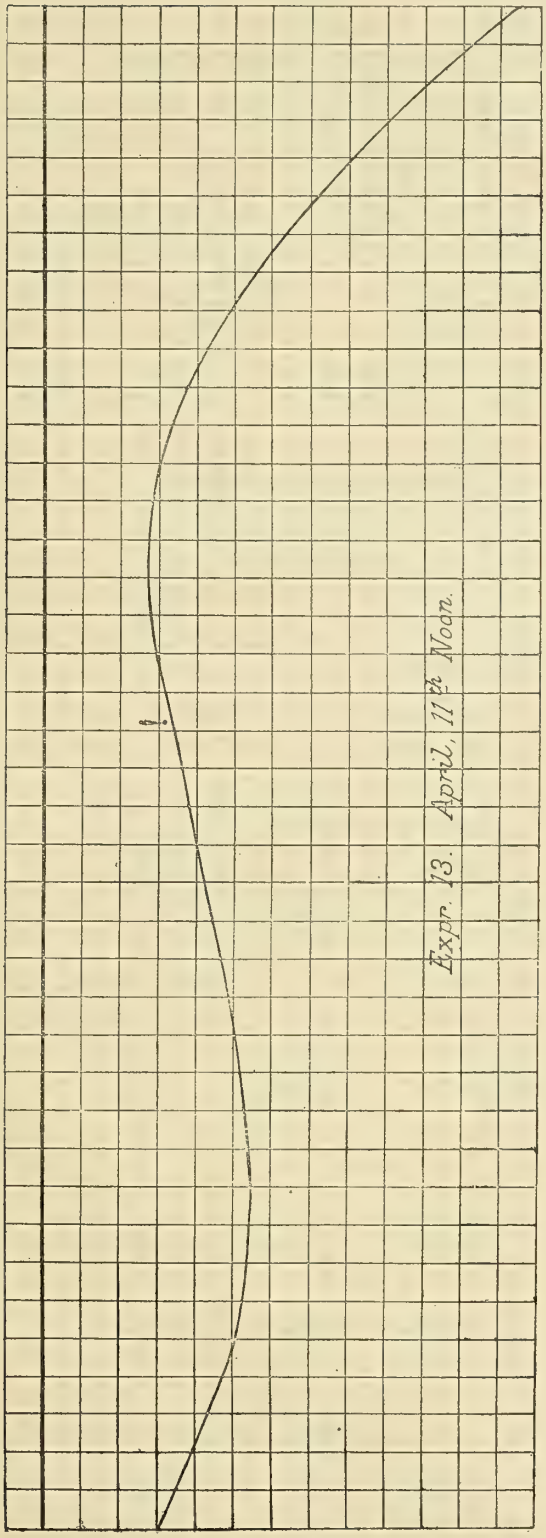
(12). *April 9th, midnight.*—$CuSO_4$, 20 grms.; H_2O, 25 c.c.



Readings: 65, 64, 62, 59, 55, 50, 48, 48, 49, 49, 50, !, 48, 44, 40, 35, 31, 27, 23, 20, 17, 16, 15, 15, 16, 18, 21, 25, 29, 33, 38, 44, 50, 56, 63, 70, 76, 82, 88, 94, 100, 105, 111, 116, 121, 124, 127, 130, 133, 135, 136, 137, 138, Stop.

In this experiment, a very small westerly torque was applied to the head one minute and a half before the release. At the moment of making contact, a westerly force was beginning, therefore, to take effect on the load; and its existence is indicated in the final coming to rest of the beam against its westerly stop. In the knowledge of this, it is apparent that an easterly force must be responsible for the easterly motion which begins immediately after contact, and persists for nearly three minutes. There was, in fact, a westerly torque of some 90 divisions of the scale at the moment of experiment.

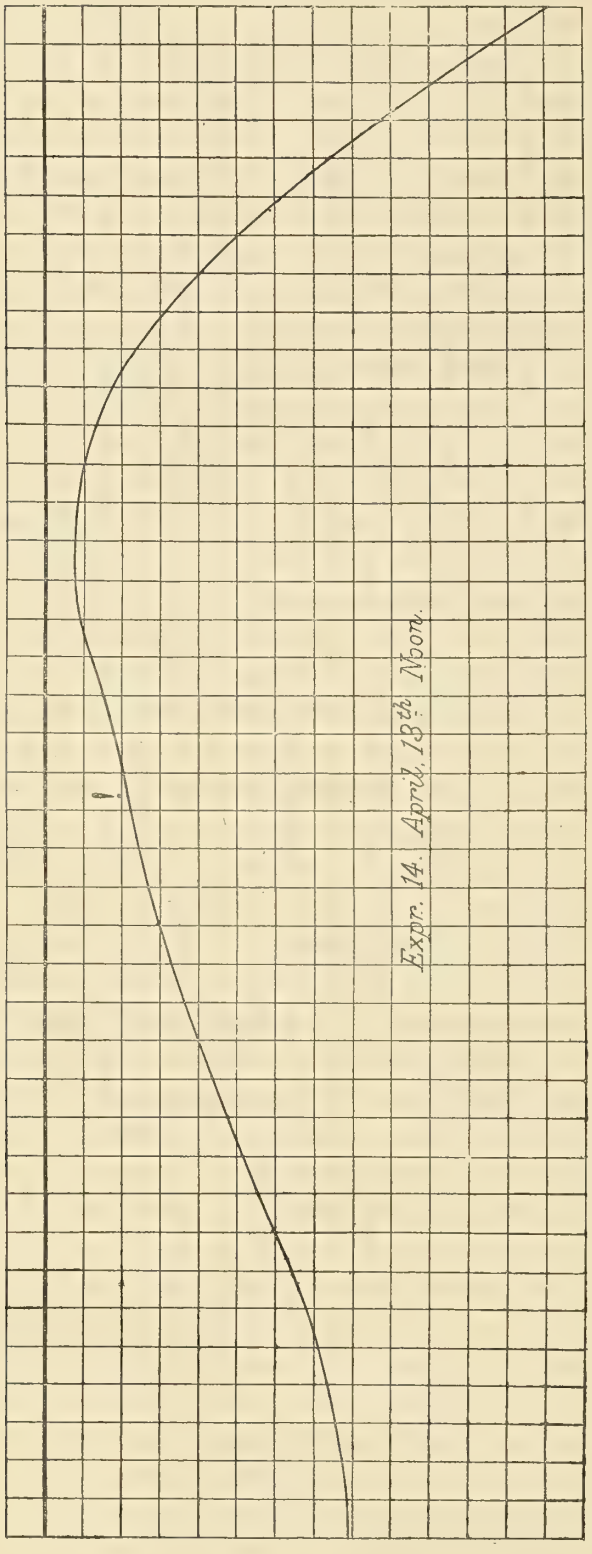
(13). *April 11th, noon.*—$CuSO_4$, 25 grms.; H_2O, 25 c.c.



Readings: 37, 35, 30, 24, 18, 11, 5, 0, *4*, *8*, *10*, *12*, *13*, *13*, *12*, *10*, *7*, *3*, 1, 7, 13, 18, 25, 31, 35, 40, 44, 48, 50, 52, 53, 54, 55, 54, 53, 52, 50, 49, 47, 44, 43, 40, 38, 36, 34, !, 32, 30, 28, 28, 28, 29, 31, 34, 38, 44, 50, 57, 65, 73, 83, 92, 102, 114, 125. Stop.

Only in part shown in curve. A very small easterly torque was applied five minutes before making contact. The experiment is made during a steady motion eastward, which becomes reversed to a marked westerly acceleration beginning one minute after contact. The beam then travels 125 to westward, and remains against stop as observed for one minute when experiment was terminated, as errors of temperature would now be probable.

(14). *April 13th, noon.*—$CuSO_4$, 25 grms.; H_2O, 25 c.c.



Readings: 90, 91, 92, 92, 91, 91, 90, 89, 88, 87, 85, 84, 83, 82, 80, 80, 79, 79, 78, 79, 79, 78, 77, 75, 73, 70, 68, 63, 59, 55, 51, 47, 43, 39, 36, 32, 29, 26, 24, 22, !, 21, 18, 15, 12, 9, 8, 7, 8, 10, 13, 18, 24, 32, 41, 51, 62, 75, 89, 102, 117, 132. Stop.

A very small torque towards the east was applied five minutes and a quarter before release. This makes the westerly acceleration, developed immediately after the descent of the water is completed, the more remarkable.

The foregoing fourteen experiments reveal certain effects on the motion of the beam apparently traceable to some force developed under the conditions of experiment. If a force is to be ascribed in any way to the motion of the Earth through a medium, it must exhibit a change of direction as regards east and west from midnight to midday. There is some indication that the noon-day experiments are attended with a more decisively westerly perturbation or acceleration than the midnight observations. The fourteen experiments may be analysed as follows, using the sign ?? to indicate a quite uncertain result, and the sign ? following the compass-direction to indicate considerable doubt as to the interpretation placed on the curve.

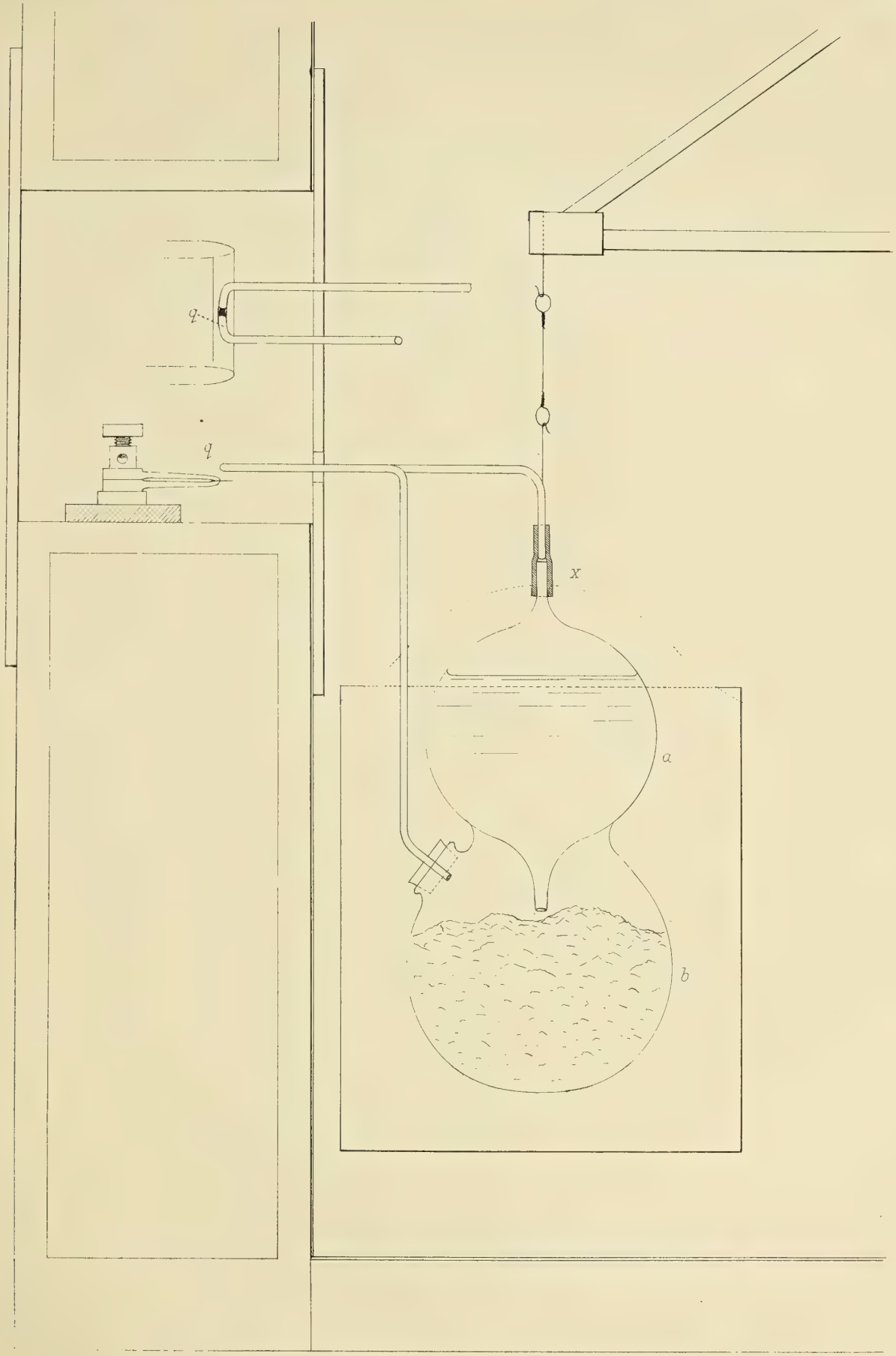
(1) Midnight: easterly (?).
(2) Noon: easterly (?).
(3) Noon: westerly.
(4) Noon: (??).
(5) Noon: westerly.
(6) Noon: westerly.
(7) Noon: westerly (?).
(8) Noon: westerly.
(9) Midnight: westerly (?).
(10) Midnight: easterly (?).
(11) Noon: westerly.
(12) Midnight: easterly.
(13) Noon: westerly.
(14) Noon: westerly.

There are thus seven results where the perturbation is most probably ascribed to a force directed westward; and these are all noon-day experiments. None of the midnight results is westerly with the same degree of probability. There are, again, no good easterly perturbations obtained at midday; while there is one very good one obtained at midnight. With reference to error from passage of heat through the cork-jacket, it is to be noticed that in all cases the final result of the experiment is indicated clearly in the second or early in the third minute from the moment of contact. It seems safe to conclude that an appreciable acceleration of the load could not arise from this source so soon after the mixing of the substances. The experiments, therefore, point on the whole to a positive result.

Further experiments are desirable, and more especially observations made at midnight. I do not think that with the present apparatus any final conclusion can be arrived at beyond that of showing that the force to be expected—if existent at all—is of very small value. It is evident that where such uncertainty as to the form of the vibration and such variation in acceleration enter the experiments, the changes of velocity noticed must be liable in many cases to misinterpretation. The conclusion may be drawn, however, *that no inertia effect corresponding to the least weight-change measurable by the most sensitive balance arises in*

these experiments. It must be remembered, too, that the space-velocity used in the forecast of the greatest possible acceleration does not include that of the solar system. According to some authorities, after allowing for the high angle with the ecliptic made by the solar motion, the velocity as used should be increased, for the time of year at which the observations were made, nearly 50 per cent.

To improve on the foregoing method of experiment the passage of heat out of, or into, the external envelope containing the vessels in which the reactions are taking place, might be further delayed by the use of a Dewar vacuum-jacket, employing very light-blown glass in its construction. The tube bearing the fusible plug might well be carried to a point beneath the centre of the beam, and there the radiator be located in a cell as far as possible cut off from the space holding the vessels. This space should be vacuous in order to reduce the mass of the convection currents, the vessels holding the reagents being, in such case, also partially exhausted. Less quantities of chemicals and smaller vessels, and, perhaps, a quartz suspension, would also be improvements. The observations should be extended to various reactions and changes of state of aggregation. So long as there was the possibility that a true weight-change had been detected by Herr Heydweiller, it appeared advisable to confine the experiments to substances which appeared to have yielded positive results.



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III.

A NEW FOUNDATION FOR ELECTRODYNAMICS.

By ARTHUR W. CONWAY, M.A., F.R.U.I.

[Read June 16, 1903.]

Our experimental knowledge of electricity can be divided into three groups—electricity at rest, electricity in slow motion, electricity in rapid motion: motions being spoken of as slow or rapid according as their velocities or accelerations are, or are not, comparable with the velocity of radiation. In the first group we have the phenomena of electrostatics; in the second we have those of currents, and probably magnetism; in the last group we have all the phenomena which appear to depend on the rapid motions of electrified corpuscles, either in orbital motions about one another, as in an incandescent gas, or else moving in a more direct path, as in a vacuum tube. Externally these motions produce the effect called radiation. To propose a theory which would include all these effects has been the object of numerous scientists. The older writers on electricity, such as Gauss, Weber, Neumann, sought for the law of force between two electrified particles, which would include the well-known law of force between two electrified particles at rest, and which would give the proper action between two currents regarded as being made up of moving electrified particles. In the treatise of Maxwell there is a gap between electrostatics and the theory of currents; whilst the latter is connected with the theory of radiation by reasoning which is open to objection. Helmholtz assumed for the electric force (X, Y, Z) the expression

$$-V^2(\partial/\partial x, \partial/\partial y, \partial/\partial z)\psi - V^2\partial/\partial t(F, G, H),$$

where $\psi = e/r$, e denoting the electric charge and r the distance of the point at

which the force is to be estimated from the charge. (F, G, H) denotes a vector potential, and is equal to $e(v_1, v_2, v_3)/r$ where (v_1, v_2, v_3) is the velocity of the charge, V being the velocity of radiation. These expressions include the laws of currents, but without modification cannot be extended to include radiation. The only successful attempt that has been made, as far as I am aware, to embrace all the facts of electricity is that of Levi-Civita,* who makes use of the fact that all these effects are propagated with velocity V, so that the quantities e, u, v, w should not have their values at the time t, but at the time $t - r/V$. The electric force then satisfied the equation $(V^2 \nabla^2 - \partial^2/\partial t^2)(X, Y, Z) = 0$. This scheme of Levi-Civita's, although complete, is open to criticism. The old school of action-at-a-distance directed attention solely to the places at which the charges were situated. Maxwell, upholding the ideas of Faraday, located everything in the medium—the æther. In the above scheme we look at regions *fixed* in the æther, and regard electric charge as something which may appear or disappear, increase or decrease, in this region. If, however, we are to regard as all-important—as we are compelled to do—the electric carrier, whether we call it ion, electron, or corpuscle, then we must look upon an electric charge as possessing an individuality of its own, and must propose a scheme which will enable us to follow each particle in its wanderings. I pass on, then, to a modification of Helmholtz's theory, which will accomplish this. Going back to the ideas of B. Riemann, the quantities which are regarded as being propagated with velocity V are potentials —scalar and vector. The scalar potential of a point-charge e at rest is Ae/r, where A is a constant depending on the units employed. Suppose, now, that it is moving through a point A, and that, after a small interval of time, dt, it is passing through B, then, at any subsequent time, the disturbance generated will be enclosed between two spheres, one having its centre at A and its radius at Vt, where V is the velocity of the radiation, the other having its centre at B and its radius $V(t - dt)$. The thickness of this shell at any point will vary, as $V/(V - v\cos\theta)$, where v is the velocity of the point-charge, when the æthereal disturbance was starting from A, and θ is the angle between the direction of motion of the particle and the line joining A to the point of the shell. We are thus led to take for the scalar potential the expression $AVe/r(V - v\cos\theta)$. Again, if there was question of finding the potential at any point (x, y, z), at a time t after starting, it is clear that the position of the particle at some previous time T

* *Il Nuovo Cimento*, ser. 4, t. vi., 1897.

is necessary, for the disturbances are not propagated instantaneously. The difference in time $t-T$ must be the same as that occupied by the radiation in travelling out a distance r. In other words, T must satisfy the equation $t-T=r/V$, where r is the distance from the position of the particle (ξ, η, ζ) at the time T to the point (x, y, z). (ξ, η, ζ) are, of course, supposed to be known functions of the time. Hence, in the expression for the scalar potential, $\xi, \eta, \zeta, v\,\theta$ are all supposed to be formed at the time T given above, The same remark applies to the expression which we take for the vector potential.

$$(F, G, H) = A\,Ve\,(v_1, v_2, v_3)/(V - v\cos\theta),$$

where v_1, v_2, v_3 are the rectangular components of the velocity v at the time T. We take, then, finally for the electric force (X, Y, Z) the expression

$$-(\partial/\partial x, \partial/\partial y, \partial/\partial z)\,\psi - \partial/\partial t\,(F, G, H).$$

In the differentiations it is, of course, to be remembered that ξ, η, ζ are functions of T, which is a function of x, y, z, t, given by

$$t - T = [(x-\xi)^2 + (y-\eta)^2 + (z-\zeta)^2]^{\frac{1}{2}}/V.$$

So that (for example)

$$\partial\xi/\partial x = \partial\xi/\partial T\;\partial T/\partial x = -\partial\xi/\partial T\,.\,(x-\xi)/r(V - v\cos\theta).$$

If we take the constant $A = V^2$, we get the electromagnetic system of units. Then the vector (X, Y, Z) satisfies the equations $\partial X/\partial x + \partial Y/\partial y + \partial Z/\partial z = 0$, $(V^2\nabla^2 - \partial^2/\partial t^2)\,(X, Y, Z) = 0$, and $\iint(lX + mY + nZ)\;dS = 4\pi V^2 e$, where the integration is extended over any closed surface containing the particle. Also, if we introduce a new vector (α, β, γ), which is the curl of (F, G, H), we find now

$$\begin{aligned} V^{-2}\,\partial/\partial t\,(X, Y, Z) &= \text{Curl}\,(\alpha, \beta, \gamma) \\ -\partial/\partial t\,(\alpha, \beta\,\gamma) &= \text{Curl}\,(X, Y, Z), \end{aligned}$$

which is the Hertz-Heaviside form of the Maxwellian equations, (α, β, γ) being the magnetic force. If we pass from a consideration of discrete point charges to a system in which the volume density is ρ, the above equations still hold outside the electricity, but inside it will be found that the first relation becomes

$V^{-2}\,\partial/\partial t\,(X, Y, Z) + 4\pi(i_1, i_2, i_3) = \text{Curl}\,(\alpha, \beta, \gamma)$, where (i_1, i_2, i_3) denotes the true current. The assumed expressions then agree with the known relations for electrostatic and radiation phenomena; whilst for slow motions we can put $V = 0$, and the expressions become those of Helmholtz, which agree with the various phenomena of electric currents.

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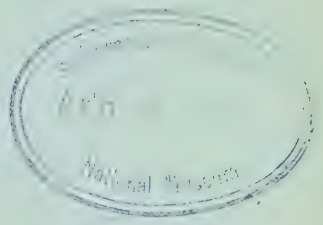

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IONIZATION IN ATMOSPHERIC AIR.

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[Read June 16, 1903.]

1.—*Introduction.*

In the last ten years we have become familiar with many methods of making a gas a conductor of electricity. A gas becomes a conductor when subjected to the action of Röntgen rays, or cathode rays, or to the radiation given off by uranium, thorium, radium, or other radio-active substance; gas drawn from an arc or a flame, or from the neighbourhood of an incandescent metal, is a conductor; so also is gas newly generated by an electrolytic cell, and other cases of conductivity in gases might be mentioned. In all these cases it has been shown that the conductivity of the gas is due to the presence in it of positively and negatively charged particles, which travel through the gas when exposed to the action of electric force, and this stream of charged particles—positive going in one direction, negative in the opposite—constitutes an electric current.

Further, it has been shown that these positively and negatively charged particles result from the breaking up of the atom into two parts, having equal but opposite charges.

This production of positively and negatively charged particles by the breaking up of some of the atoms is called the ionization of the gas. When a gas is ionized, even a very weak electric force sets the ions in motion, and produces an electric current; in the normal state of a gas, when not ionized, we can only force an electric discharge through it by using very strong electric force, sufficiently strong to cause directly or indirectly the splitting up of the atom.

Up to a short time ago we looked upon a gas before it had been exposed to any of the ionizing agencies mentioned above, as absolutely a non-conductor; but in 1900, C. T. R. Wilson* in Cambridge, and Geitel† in Germany, showed that there is always a slight ionization in atmospheric air.

* Proc. Camb. Phil. Soc. † Physikalische Zeitschrift, 2. Jahrgang, No. 8, pp. 116–119.

The experiments described in this paper have to do with this slight ionization existing in atmospheric air in its normal condition. In addition to papers by Wilson, and by Elster and Geitel, the subject has been dealt with by Prof. Rutherford* of Montreal, Prof. McLennan and Mr. Burton† of Toronto, and by the Hon. R. J. Strutt.‡

2.—*Description of Apparatus.*

A square piece of thick wood *ABC* (fig. 1) was taken and covered over both faces with tinfoil connected to earth. On this tinfoil coating rest blocks of paraffin *FG*, supporting a plate *L*, 25·5 cms. by 25·5 cms., made of perforated zinc. A second similar plate *M* is supported by the metal stem *H* from a clamp (not shown in figure), resting on paraffin blocks on the tinfoil coating of *ABC*. Pieces of paraffin *DE* are inserted in the wooden base, care being taken that they are surrounded by tinfoil in contact with the earthed coating of the base; through these blocks wires pass, making connexion with the plates *L* and *M*.

One plate *M* is charged, say positively, *L* being to earth, and then *L* is disconnected from earth, and slowly rises in potential, due to the passage of a current through the gas from *M* to *L*. It is obvious from the arrangement that this rise in potential of *L* must be due to a current through the gas; a leakage by the supports *FG* could not produce the effect, as these supports rest on a conductor kept at zero potential.

A leakage over the supports *FG* would diminish the rate of increase of potential of *L*, but could not possibly produce the rise. We are safe, therefore, from the error of ascribing to ionization of the air effects due to faulty insulation; the only error that can occur is that of getting too small a value of the ionization. This is guarded against by testing the insulation, and in fact it is quite easy to get the insulation perfect, as it is only subjected to quite a small electric field.

The rate at which the plate *L* gets charged is measured by a quadrant electrometer *E*. One pair of quadrants is permanently connected to earth through the mercury cup *c* in a paraffin block; the other pair of quadrants is permanently connected to the plate *L*, and can be put to earth, or insulated, by putting in or taking out the metal piece *d* connecting the mercury cups *b* and *c*. This connecting piece *d* can be operated by a string from a distance from which the scale is observed by a telescope to avoid induction effects produced by the motion of the observer. The paraffin block *a*, and all insulating supports used to carry the wire joining *E* to *L*, are set on sheets of tinfoil connected to earth, to avoid all possibility of the

* Phil. Mag., Dec., 1902. † *Ibid.*, June, 1903. ‡ *Ibid.*, June, 1903.

potential of L changing from zero, except by a current through the surrounding gas. For measuring the leakage through air in this way a very sensitive electrometer is required; but I have had to be content so far with an instrument giving a deflection of 60 mm. scale divisions for 1 volt difference of potential between the quadrants. To get deflections of sufficient magnitude it has been necessary to lengthen the time of each observation—in general the deflection produced in four minutes has been observed. When care is taken to have the

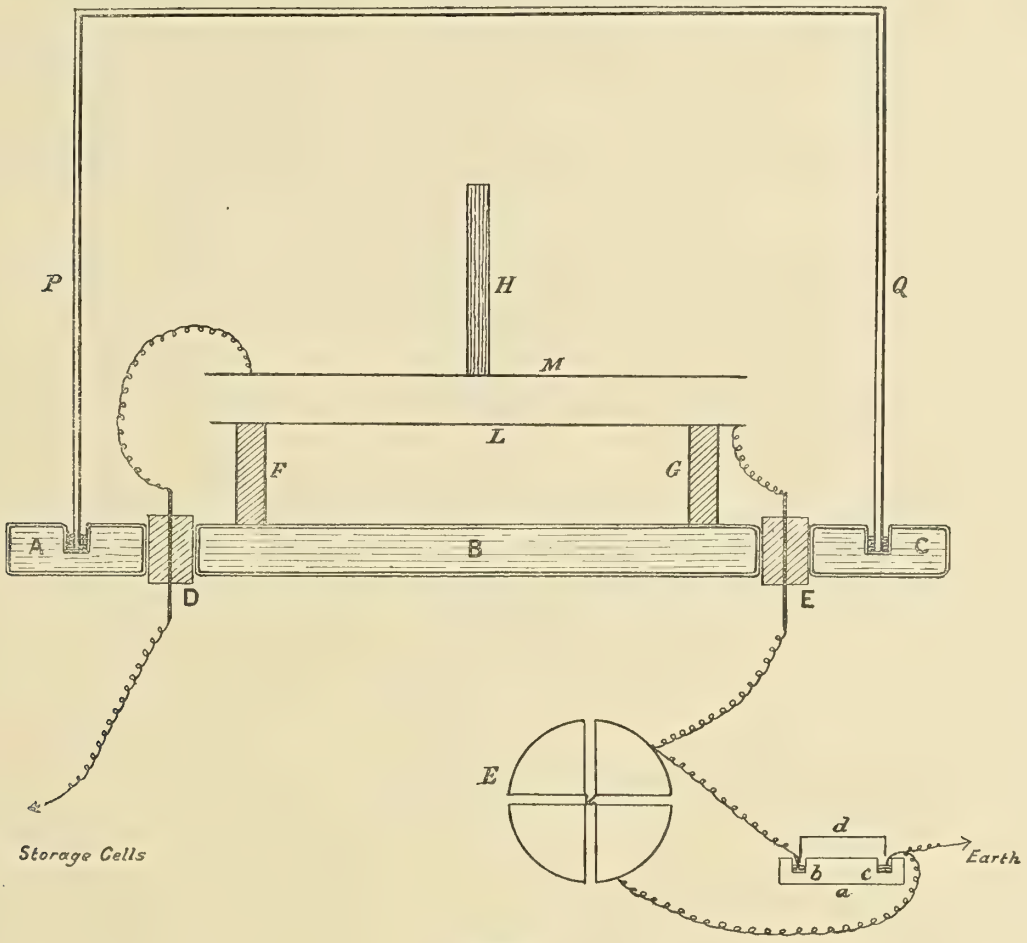

Fig. 1.

electrometer free from vibration, and properly shielded from electric disturbances, it is quite easy to get the zero sufficiently steady to admit of taking readings extending over four minutes or more. Still it involves more time, and, in addition, there are many obvious experiments I have been unable to make.

A glass case PQ, lined inside with tinfoil, can be placed over the plates L and M, resting in a groove in the base ABC, filled with mercury or glycerine making the whole practically air-tight.

3.—*Nature of the Conductivity in Atmospheric Air.*

It is scarcely necessary to repeat observations showing that the conductivity in atmospheric air is of the nature resulting from ionization of a gas. The relation between current and E.M.F. is such as is produced by ionization. One curve is given (fig. 2), showing the variation of the current with the potential difference of the plates between which the current passes through the air.

In this connexion we may describe another experiment. The glass case PQ (fig. 1) is removed, and replaced by a case of perforated zinc (fig. 3); a second case RS, similar, but slightly larger, also of perforated zinc, is placed enclosing the first, and resting on paraffin supports. The whole apparatus is placed where a gentle draught of air is blowing, so that a stream of air passes through the perforated covers, and reaches the plates L and M. The plate M is charged positively,

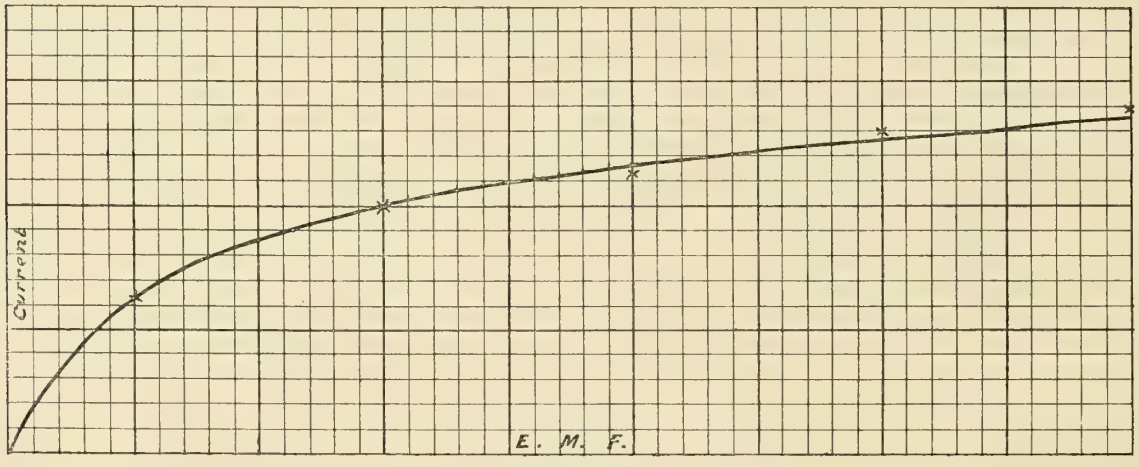

Fig. 2.

and both covers connected to earth; the current observed is 5 (in arbitrary units). The inner cover PQ is kept to earth, and RS joined to the storage cells so as to be 160 volts positive; this produces an electric field, assisting the positive carriers to reach the inside of the cover PQ, and the current is found to be increased to 7. The inner cover is kept to earth, and the outer kept at 160 volts negative. This prevents the positive carriers from traversing the space between the covers with the stream of air entering the inner cover, and the current between M and L is now found to be only 0.5 on the same scale as before. Similar numbers are obtained when the plate M is charged negatively, only now the small current occurs when the outside cover is positive, and the large current when the outside cover is negative. Such an experiment is easily understood when we consider that the observed conductivity is due to positively and negatively charged particles, but would otherwise be difficult to explain.

4.—*Amount of Ionization in Atmospheric Air.*

It is possible that the amount of ionization in atmospheric air may be of great importance as a meteorological factor, and it is, therefore, important to know how the amount of ionization changes from time to time. We know, for example, from the work of C. T. R. Wilson, that charged particles, such as the ions resulting from the ionization of a gas, can act as centres of condensation, and take the part formerly assigned entirely to dust particles, of acting as condensation nuclei.

It is, therefore, obvious how important may be a supply of such ions in causing condensation of water-vapour in the form of rain.

Again, the amount of ionization in the atmosphere, with the accompanying currents to or from the Earth's surface, may be of importance, as affording an explanation of the variations of the Earth's magnetism.

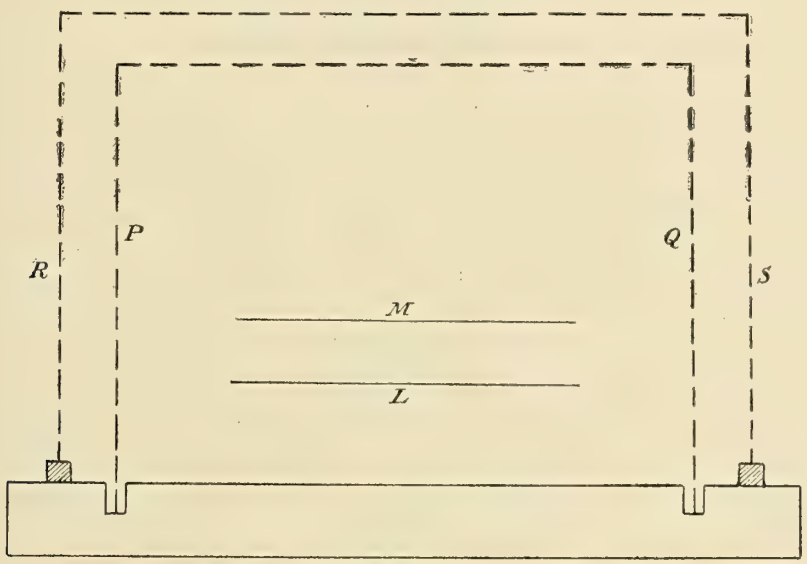

Fig. 3.

There are various methods we may follow in observing the amount of the ionization in the air. We may enclose the apparatus in an air-tight case, and observe the current. In this case we get the rate of production of ions, and not the actual amount at any time. The actual amount of ionization at any time depends not only on the rate of production of ions, but also on the rate of loss resulting from recombination of positive and negative, and from other causes. Wilson's experiments give the rate of production of ions in a closed vessel as about 15 per second per cubic centimetre. Rutherford has made some experiments, in which he draws a stream of air at a known velocity through a tube and through his measuring apparatus, and the electric current observed gives the ions in a cubic centimetre of air. This

number comes out surprisingly small; values were found varying from 13 to 40 ions per cubic centimetre; that is, on some days as small as the number of ions produced per second in a closed vessel.

As Rutherford himself shows, we should expect that the total ionization at any instant would be very much greater than the number of ions produced per second.

If recombination were the only source of loss of conductivity, the number (n) of ions per c.c. would increase until the rate of production (q) is equal to the rate of loss, or

$$q = \alpha n^2$$

where α is a constant.

The constant α has been determined by various experimenters in the somewhat similar case of ionization by Röntgen rays, and we may take it as 3500 E, where E is the charge on the ion, or $3{\cdot}4 \times 10^{-10}$. Substituting this value for α and for q its observed value in closed vessels, viz. 15, we obtain

$$n = 3800.$$

Of course the value assigned to α may be considerably in error, when applied to the case under consideration, and there are other sources of loss of ionization in addition to recombination. Besides, if a considerable percentage of the ionization inside closed vessels is due to a radiation from the walls of the vessels, as may follow from the work of Strutt and M'Lennan, the rate of production of ions in such vessels may be greater than in the same volume of free air. Still, we should expect the number of ions per c.c. near the surface of the Earth to be much greater than the number produced per c.c. per second in a closed vessel. To test this point some observations have been made with the apparatus described in fig. 1; the plates between which the current passes were freely exposed to the air of the room, the cover PQ having been removed.

If the room is fairly air-tight, the current, as a rule, rapidly diminishes from the value it has when fresh air has just been admitted, the rate of production of ions being less than the loss. Cases occurred in which this diminution was not so marked; this occurred where the ionization in the free air was below the average. Observations were made with the room not closed, doors and windows being opened in such a manner as to maintain a steady renewal of the air without any appreciable draught.

With such an arrangement an electrometer deflection of as much as 20 scale divisions in 4 minutes was often obtained. The electrometer gave a deflection of 60 scale divisions for one volt potential difference between the pairs of quadrants, and the capacity of the electrometer and connected system was 62 electrostatic

units. Taking the charge on an ion as $3{\cdot}4 \times 10^{-10}$ electrostatic units, the observed current would mean the using up of 8×10^5 ions per second from the volume of air between the plates L and M, or 680 ions from each c.c., the volume between the plates being 1170 c.c.

If there were only about 15 ions per c.c., this would mean a much more rapid renewal of the air than could have been taking place.

To prevent the possibility of ions travelling up to the apparatus under the action of the electric field, and thus drawing the supply from a larger volume of air, the apparatus was enclosed in a case of perforated tinfoil connected to earth. This diminished the electrometer deflections about 25%.

It is intended to make an accurate determination of the number of ions in free air, but the above experiments show that the number is usually very much greater than that observed in closed vessels, where only the rate of production is measured.

5.—*Variations of the Amount of Ionization in Atmospheric Air.*

A large number of observations of the amount of ionization at different times was made. The variations were found to be very considerable, the maximum ionization observed being about $2\frac{1}{2}$ times the minimum. The greatest ionization observed was after several hours' steady rain. This agrees with the known radio-activity of freshly-fallen rain. Wilson* has shown that the residue obtained by boiling off some freshly-fallen rain emits a radiation capable of ionizing a gas. After several hours' rain we may, therefore, look upon the surface of the ground as strongly radio-active, and the large ionization observed would thus be accounted for.

On the other hand, the smallest ionization observed was after a slight shower; this is probably due to the ions having been removed from the air by the condensation on them of water-vapour. Apart from these extremes of maximum and minimum, it is difficult to account for the variations in the amount of ionization. Quite rapid variations occur; a 25% change in one hour often occurs. On a clear, bright day, with little wind, the amount of ionization is usually very steady.

6.—*Causes of Ionization.*

By enclosing air in a vessel, and lining the interior with various substances, Strutt† has shown that all the substances tested emit a radiation capable of

* Proc. Camb. Phil. Soc. Vol. xi., Pt. vi. † Phil. Mag., June, 1903.

ionizing a gas. This would suggest that the ionization in the atmosphere may be due entirely to a radiation from the surface of the Earth. However, the radiation observed by Strutt is weak, and would not seem sufficiently to account for the atmospheric ionization. Wilson's results, showing the radio-active properties of the residue obtained by boiling freshly-fallen rain, point to the presence in the atmosphere of some strongly radio-active substance, and we have seen above that the greatest ionization occurs when this radio-active substance has been deposited on the surface by a heavy fall of rain. The facts, on the whole, point to some radio-active substance disseminated through the atmosphere.

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[December, 1903.]

THE

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OF THE

ROYAL DUBLIN SOCIETY.

VOLUME VIII.—(SERIES II.)

V.

THE TOTAL SOLAR ECLIPSE OF 1900.

Report of the Joint Committee appointed by the Councils of the Royal Dublin Society and Royal Irish Academy.

Plates VI.–VIII.

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Report of the Joint Committee appointed by the Councils of the Royal Dublin Society and Royal Irish Academy.

Read before a joint Meeting of the Royal Irish Academy and Royal Dublin Society, March 27, 1901.

(PLATES VI.–VIII.)

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I.

Professor C. J. Joly's Report.

Origin of the Expedition.

In the summer of the year 1899, Sir Howard Grubb proposed that a Joint Committee should be appointed by the Councils of the Royal Dublin Society and the Royal Irish Academy to consider the feasibility of fitting out an expedition to observe the Total Solar Eclipse of 1900, May 28th. As many members of the Societies signified their intention of witnessing the eclipse, it seemed probable that the number of those joining the expedition would be sufficiently large to justify the chartering of a steamer, and it was believed that the greater part of the cost of the instrumental equipment might be defrayed by fixing the prices of the berths slightly in excess of the contract charge.

The Committee was duly elected, and 130 provisional applications for berths were received; but when it became necessary to ask for definite promises, only thirteen members ratified their applications. The Committee attributed this extraordinary falling off in numbers to the unsettling effect of the war in South

Africa. The project of chartering a steamer had consequently to be abandoned, but the Committee recommended that observations of the eclipse should be undertaken, and that the Societies should provide the necessary instruments. Accordingly the Societies placed at the disposal of the Committee a sum of money sufficient to procure certain of the instruments employed in the observations described in this Report, and to cover the cost of freightage. These instruments will be available on future occasions, and it may confidently be hoped that other expeditions will take place under the auspices of the Royal Dublin Society and the Royal Irish Academy.

Some of the apparatus was kindly lent by Sir Howard Grubb and by Mr. W. E. Wilson. There remain in the possession of the Societies two 8-in. mirror cœlostats and a photographic lens of 4 in. aperture and of 19 ft. 4 in. focal length.

Selection of a Station.

Owing to the protracted nature of the negotiations referred to in the last section, but little time was left for the preparation of the instruments and the choice of a station. Finally, the Committee gladly accepted the kind invitation of Senor Don Iniguez y Iniguez, Director of the Madrid Observatory, and resolved to occupy a station in proximity to that selected by the Spanish Government expedition from Madrid. The Spanish camp was situated on the summit of the little stony hill of Berrocalillo, about half an hour's walk from the town of Plasencia, and the instruments of the Irish expedition were erected just below the summit on the southern slope. The longitude of the station is 6° 6′ 43″, and the latitude 40° 2′ 5″. Mr. Rudolf Grubb arrived in Plasencia early in May, to assist in the erection of the Spanish instruments and of the supports for our cœlostats. Our instruments did not reach the station until May 23rd, the day before we arrived.

Our best thanks are due to Senor Iniguez and his colleagues for the great assistance they afforded us. Without their aid, and in the short time at our disposal, it would have been almost impossible for us to have erected our instruments in such an out-of-the-way place. They procured the services of the local carpenter for us, and we had the advantage of sharing the attention of the town-guard in keeping the camp free from trespassers. It is true that the situation was rather exposed and inconveniently distant from the town, and a strong wind on the day of the eclipse might have caused much mischief, especially as we were not provided with huts; but under the circumstances it was quite out of the question to select another camp and set about preparing new supports for the cœlostats.

Instruments.

As Sir Howard Grubb gives a detailed account of the instruments in a subsequent part of this Report, it is only necessary for me to state that they consisted of—

(1) A cœlostat and photographic camera, fitted with a corrected lens of 4 in. aperture and of 19 ft. 4 in. focal length, for photographing the corona;

(2) A cœlostat and photographic camera, with a lens of 6 in. aperture and 7 ft. $10\frac{1}{2}$ in. focal length, for photographing the corona through a coloured screen;

(3) A spectroscopic apparatus, for securing a continuous series of photographs of the "flash spectrum."

Members of the Expedition.

The following accompanied the expedition :—

Prof. Charles J. Joly, M.A., F.T.C.D. (*Chairman of the Committee*).
Sir Howard Grubb, F.R.S.
Dr. A. A. Rambaut, F.R.S.
Mr. W. E. Wilson, F.R.S.
Prof. Bergin, M.A.
Mr. Samuel Geoghegan, C.E.

We met Mr. Rudolf Grubb at Plasencia, and were joined on the 27th by Dr. Downing, F.R.S., the Superintendent of the Nautical Almanac Office, who kindly gave the signal *Start* at the commencement of totality, and the signal *Close cameras* 85 secs. after. The second signal was actually given some seconds after the end of totality, as the duration of the eclipse was less than its predicted extent, being only 82 secs. according to Dr. Downing's observations. During totality the seconds were counted by Mr. Rudolf Grubb, using a metronome. Prof. Bergin assisted me in the adjustments of the long-focus camera, and uncapped for the exposures. Mr. Wilson manipulated the camera with the coloured screen, and Sir Howard Grubb and Dr. Rambaut worked the spectroscope. Dr. Downing and Mr. Geoghegan observed the shadow-bands.

A preliminary account of the results of the expedition was presented to a joint meeting of the Royal Dublin Society and the Royal Irish Academy, held on April 4th, 1901.

Long-focus Camera.

The adjustments of the cœlostat call for no special remarks, as the subject is treated at great length in most of the recent reports of expeditions fitted out

by the Royal Society and the Royal Astronomical Society. The long wooden camera was mounted on wooden supports, and was covered with dark American cloth turned inside out. Some trouble was experienced owing to dust being blown into the clock-work, and to an instability in the adjustments, due apparently to warping of the apparatus in the strong sun. After a little experience it was easy to make the slight adjustment required to keep the image fixed for a considerable time, and the adjustment seems to have been good during the exposure of the plates.

Two photographs of the corona were taken on Sandell's triple-coated plates, and they were developed by Mr. McGoogan, photographer to the Dublin Science and Art Museum, who also prepared the transparencies deposited in the rooms of the Royal Dublin Society and of the Royal Irish Academy.

The first plate received an exposure of 50 secs., and the second an exposure of about 15 secs. Owing to the actual duration of totality being shorter than that predicted, I saw the returning burst of sunlight before the camera was closed after the second exposure. The strong light has caused a reversal on one portion of the plate. Both negatives exhibit a faint secondary image, due to reflection from the lens. This image was clearly visible on the ground focussing-glass when the image of the sun was cast upon it. I am much indebted to Mr. W. H. Wesley, Assistant-Secretary of the Royal Astronomical Society, for his great kindness in writing the annexed description of the negatives. Mr. Wesley is well known as one of the greatest authorities on photographs of the corona.

Description of Photographs of the Corona.

(By Mr. Wesley.)

Plate A (50 secs. exposure). This plate shows a coronal extension of more than a lunar diameter in the north-west, of about a diameter on the east side, and of more than a diameter in the south polar region; in the north the extension is a little less. The prominences are well defined, showing the focus to have been good, but there is a certain haziness about the photograph generally, rendering the details somewhat indistinct; this is especially shown in the fine rays filling the north polar rift, which are very ill-defined as compared with Plate B. The sky is a little fogged, as was to be expected with so long an exposure.

Plate B (15 secs. exposure). The coronal extension is about three-fourths of a diameter on the west, about half a diameter on the east, and more than one-third diameter in the polar regions. The extension is thus less than in Plate I, but the definition is much finer, and the corona close to the limb appears quite as dense as in the other plate. The fine rays which fill the north polar rift are

very well defined, and there are clear indications of the interesting arching structures of the corona about the great prominences in the west and south-west. A small portion of the reappearing Sun (or very bright chromosphere) on the west is sharply reversed, but has scarcely had any effect in fogging the plate. The prominences are finely shown, and are more distinct than in Plate A.

Both negatives are somewhat marred by a shift of the plate, producing a second image of the brighter portions, but they are otherwise very clear and free from defects. The polar diameter of the Moon's image is 53 mm. (= 2·1 in.): the equatorial diameter is slightly less, owing to the motion of the Moon during exposure.

The corona of 1900 belongs to the type which appears to have always accompanied a minimum Sun-spot period, though not so markedly as in 1878, and January, 1889.

The polar rifts are widely opened and filled with the fine rays so generally seen in these regions at minimum periods. They are far finer and more delicate about the north pole than at the south, where they are more diffused and indistinct.

With regard to the main equatorial masses of the corona, the edges bounding the western side of the north and south polar rifts are much less inclined from the radial in an equatorial region than the corresponding edges on the eastern side. The corona therefore is more spreading on the west and more pointed on the east, producing the general resemblance to the form of a wind vane that has been so often noted. This is of course best shown on Plate VI., with the longer exposure: this plate also shows the characteristic curve of double curvature of the edge bounding the north polar rift to the west. There are clear indications of interlacing or arching structure in the corona on the west side, in the region of the large prominences, in contrast with the more uniform and quiescent appearance of the eastern side, where the prominences are smaller and less conspicuous.

The prominences are well shown on both plates, the larger being on the west or south-west. One of these forms a kind of arch, as though two adjacent prominences were united by a band across their summits.

Remarks on the Reproductions.

Owing to the difficulty of reproducing the prominences and coronal extensions on the same plate, two plates have been made from negative B—the photograph taken towards the conclusion of totality with an exposure of about 15 secs. Plate VI. gives a good general idea of the photograph; and the structure of the

corona is well shown, although, of course, the finest details are lost, and there is less extension than on the negative. The larger prominences are more clearly seen on Plate VII. Both plates exhibit "Baily's beads"—the bright, isolated points of returning sunlight appearing as dark dots in the reversed portion of the photograph.

Miscellaneous Remarks.

The day of our arrival at Plasencia was much overcast, and the weather had been uncertain for some time previously. Subsequently the sky was almost absolutely free from clouds, and the transparency at night was very unusual to us, accustomed to the hazy skies of our climate. The day of the eclipse was not quite so perfect as those preceding, and a few clouds were visible on the horizon. The first signal of the immediate approach of totality was caused by the fading of a white cloud-bank, which lay in the track of the shadow, owing to the diminution of contrast with the darker sky above. The shadow itself was invisible both at commencement and close of totality. The position of our station was singularly favourable for witnessing the changes of colour in the sky and over the landscape, and the effects were marvellously beautiful. During totality colours were clearly discernible, and the light must have very considerably exceeded that of the full Moon. The view from the hill of Berrocalillo is very extensive, the chief features being the picturesque little town of Plasencia towards the east, and the snowy summits of the Sierra de Gredos on the north-east; but during totality the light was quite sufficient to distinguish most of the details.

II.

Mr. W. E. Wilson's Report.

The point which I wished to determine was whether it would be possible, by suitably coloured screens, to find how far the real corona extends from the Sun, and how much of what we see is due to the atmospheric glare. Part of the light of the true corona seems to be monochromatic, giving one bright line in the green at 1474. If therefore a suitable screen could be found that would cut off all the spectrum except that particular ray, we ought to get a true photograph of the corona, so far as it is composed of coronium.

Experiments were made in my laboratory on different screens, and finally one was fixed upon which cut off the entire ultra-violet end of the spectrum up to the edge of the green. It was made by taking an ordinary cut celluloid film, $8\frac{1}{2}$ by $6\frac{1}{2}$, and clearing it in the hypo-bath. After being then well washed with

water, it was soaked for a few minutes in a bath of tartrazine, and allowed to dry. It then was a fine yellow colour, and transmitted the green, yellow, and red part of the spectrum. As the photographic plate was made especially sensitive to the green, and not much to the yellow and red, it was not thought necessary to try to cut off that end of the spectrum by another screen of suitable colour.

The image of the Sun was formed by an object-glass of 6 inches aperture and 7 feet $10\frac{1}{2}$ inches focus, mounted at one end of a long wooden tube, which, at its other end, carried a plate-holder taking a plate $8\frac{1}{2}$ by $6\frac{1}{2}$. The light was fed into this camera by a fine cœlostat, made by Sir Howard Grubb, and kindly lent to me by the Royal Dublin Society and Royal Irish Academy. The plate was exposed for 80 secs., which was 5 secs. short of the calculated time of totality, but which turned out to be 3 secs. shorter. I developed the plate on my return home. I think it shows more detail up to the Sun's limb than if the plate had been exposed without the screen, and also considerable extension in the outer corona (see fig. 3, Pl. VIII.). It would have been of more interest if another photograph could have been taken simultaneously with another similar instrument without a screen.

The spectroscopic observers report that the bright line in the green, due to coronium, was very faint in this eclipse, and in fact could not be seen by some.

The planet Mercury appears on the plate.

III.

Sir Howard Grubb's Report.

The Instrumental equipment of the Expedition consisted of one cœlostat, worked in conjunction with a 6-in. object-glass of about 8 ft. focus, mounted on a camera, and used by Mr. W. E. Wilson for producing photographs through his yellow screen:—

A second cœlostat, worked in conjunction with a specially corrected 4-in. object-glass of 19 ft. focus, mounted in a long wooden tube provided with a camera, and used by the Astronomer Royal for Ireland, and Prof. Bergin, for producing the large pictures:—

An instrument of the kinematograph type, for producing a series of pictures of the spectrum of the chromosphere, etc., worked by Dr. Rambaut and Sir Howard Grubb.

The only instruments that possess points of novelty and need be described are the cœlostat and the kinematograph instruments.

One of the cœlostats is shown in fig. 1.

The principal feature of this instrument is that (in view of its possible use on future occasions) it is made adjustable to various latitudes, and the clock is so mounted on the framework that the adjustment for latitude can be made without in any way interfering with, or putting out of adjustment, the necessary gearing between the clock and the driving-screw.

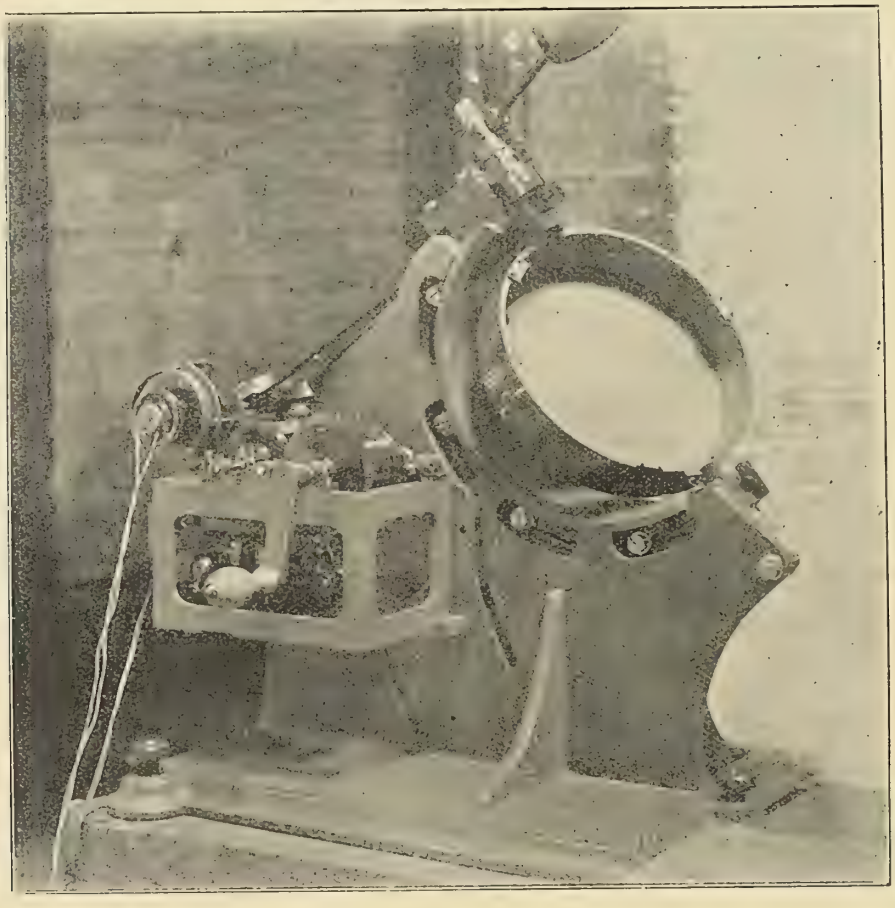

Fig. 1.

It will be seen by reference to the photograph that the clock-frame is pivoted on a bush, which is centrical with the screw-spindle, and is also connected to the frame by a series of parallel levers, so that it must in all positions be properly geared to the screw-spindle, and also must retain its horizontality for every latitude for which the instrument has to be adjusted.

Kinematograph.

The conditions required to be fulfilled by this instrument were somewhat difficult.

It was required that some twelve photographic plates should be exposed to the image of the spectrum during about the same number of seconds, and that there should be absolutely no interval between the successive exposures, so that if any flash-lines made their appearance, even for a moment, during those 12 secs., their images should certainly be impressed on some one of the plates.

In any ordinary form of kinematograph there must be a sensible interval between the exposure and each successive plate; therefore it was necessary to devise a special apparatus to fulfil the above conditions.

A strong steel shaft is mounted in the centre of a camera-box, passing vertically through stuffing-boxes in top and bottom, so as to be capable of smooth and easy motion in a vertical direction. An arm on this shaft, working against a knife-edge, with spring to keep the arm in contact, prevents this plunger from turning on its axis, or from any motion except one purely vertical, and this plunger carries a pair of silvered mirrors, long and narrow in form, and mounted over one another, with their reflecting surfaces at right angles to each other. (See figs. 4 and 5.)

These mirrors are placed in the path of the rays, between the object-glass and prism, and the image as formed on the photographic plate.

When the plunger is at its highest position, the rays fall on the lower mirror, and are directed at right angles to one side of the camera, while in its lowest position the rays fall upon the upper mirror, and are directed to the other side of the camera, and on to the other photographic plate. Moving the plunger therefore up and down has the effect of throwing an image of the spectrum alternately on each plate, and in an intermediate position partly on both plates, and there is absolutely no interval of time in which the spectrum is not thrown on one or other.

The hexagon drums which carry the six plates are attached to each side of the camera in slides, and by a simple piece of mechanism each drum is moved forward a sixth of a turn during the time that the plate on the opposite drum is exposed, so that by the time the other plate has been exposed, and the mirrors shifted, a fresh plate is presented to the image on the first side, and so on. The movement of the mirrors in a vertical direction being absolutely in the plane of the mirrors themselves, produces no shifting of the image on the plate, and therefore no indistinctness.

The mechanism by which these motions are effected can best be understood by reference to figs. 3, 4, 5, and the accompanying description; and the chart, fig. 2, will show very clearly the effect of the arrangement. This chart is drawn out on Babbage's system of notation, representing horizontally a single operation corresponding to two turns of the handle, which handle is turned uniformly.

It will be observed that during the exposure of the plate the drum on which it is fixed is stationary, but the other drum is in the act of being turned round, so as to have a fresh plate ready for the next exposure on the other side. It

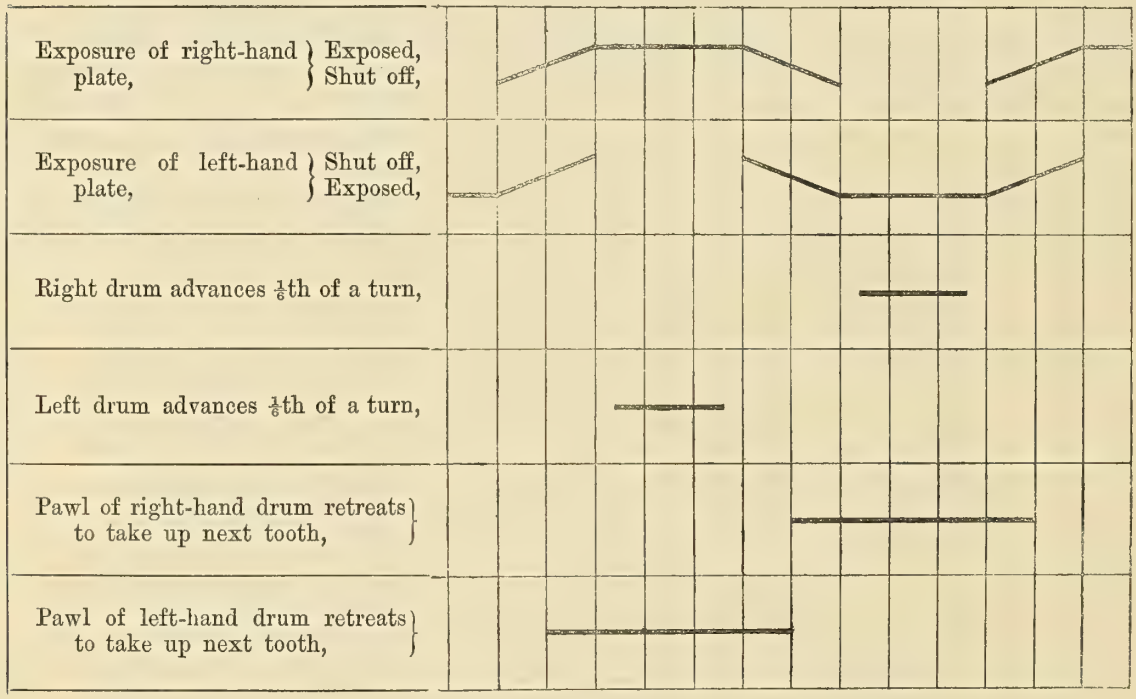
Exposure of right-hand plate, } Exposed, } Shut off,
Exposure of left-hand plate, } Shut off, } Exposed,
Right drum advances $\frac{1}{6}$th of a turn,
Left drum advances $\frac{1}{6}$th of a turn,
Pawl of right-hand drum retreats to take up next tooth,
Pawl of left-hand drum retreats to take up next tooth,

Fig. 2.

will also be observed that there is a lapping between the exposures of the two plates, that is to say, the plate at the right-hand side has a portion of the spectrum thrown upon it before the image is cut off from the left.

The results obtained appear to show that an instrument of this sort would be valuable on future occasions, but that it would be necessary that it should be made stronger, heavier, and more stable than that used at Plasencia. This latter instrument was made up hastily, just before starting for Spain, and made mostly of wood, the result being that, although it was loaded with heavy stones during the operation, some of the photographs suffered considerably from want of perfect steadiness.

Description of Figures.

The kinematograph arrangement used is shown in detail in figs. 3, 4, and 5.

In the end view (fig. 3) is seen the handle *h*, two turns of which are required for each exposure, actuating a crank and connecting-rod which drives the lever *l l* up and down. The fulcrum of this lever being some four feet from the portion shown in the figures, the motion is almost purely vertical.

This lever actuates the piston *c c* (see figs. 3, 4, 5), which carries across it a light steel bar, *b*, *b*, *b*, *b*, shown dotted in end view, and is connected by cords

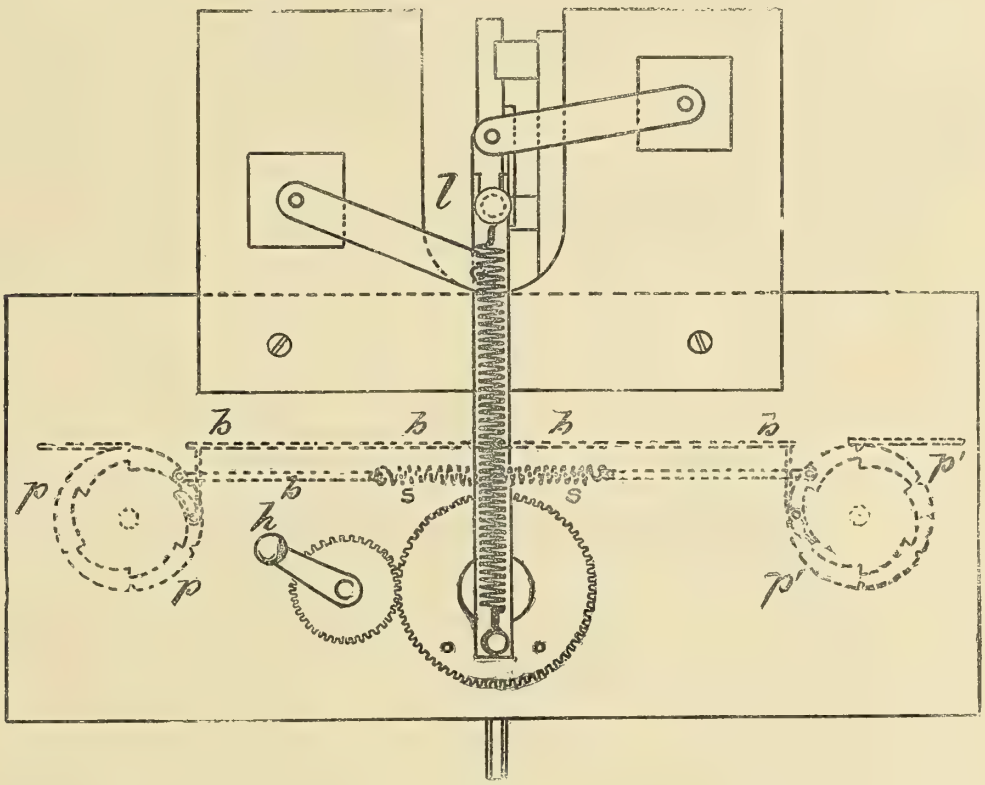

Fig. 3.

to the pulleys *p p*, *p′ p′*, which carry pawls, engaging in six-toothed rachets on the drums carrying the photographic plates.

A second pair of cords, connected by a spring *s s*, serves to keep the teeth of the ratchets pressed against the pawls.

As the bar *b b* is carried up and down, the effect is to advance the drums one-sixth of a turn alternately. The left drum advances one-sixth as the bar is raised (the right remaining stationary), while the right advances one-sixth while the bar is lowered.

Attached to the lever *l l* is a secondary lever *l' l'*, coupled to the plunger *p p* by a pin working in a slot, and the plunger carries a pair of diagonal mirrors *m m*, *m' m'*.

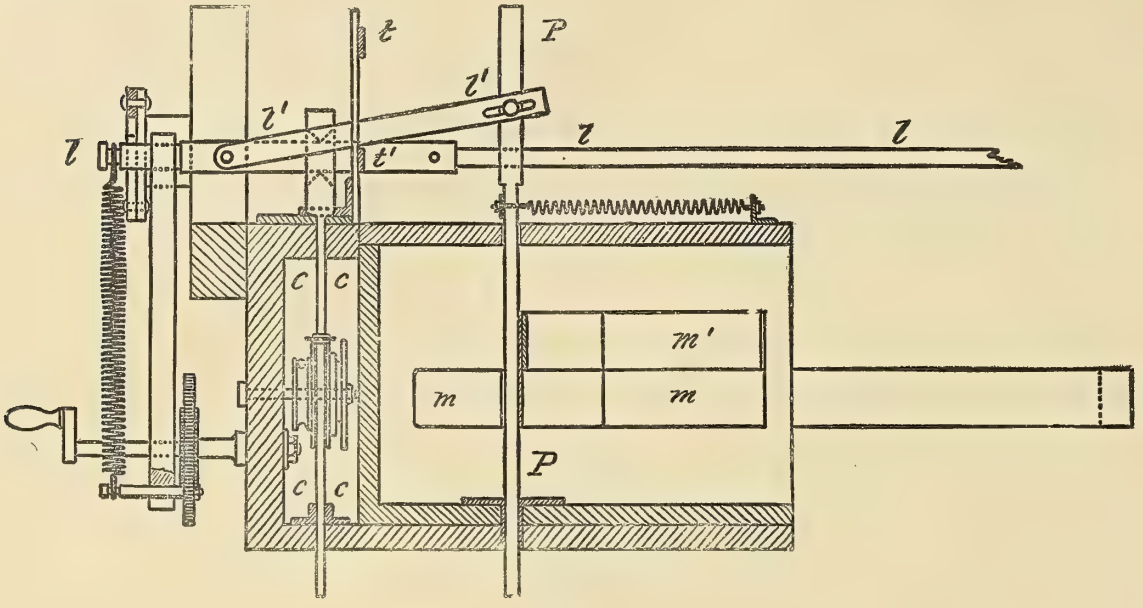

Fig. 4.

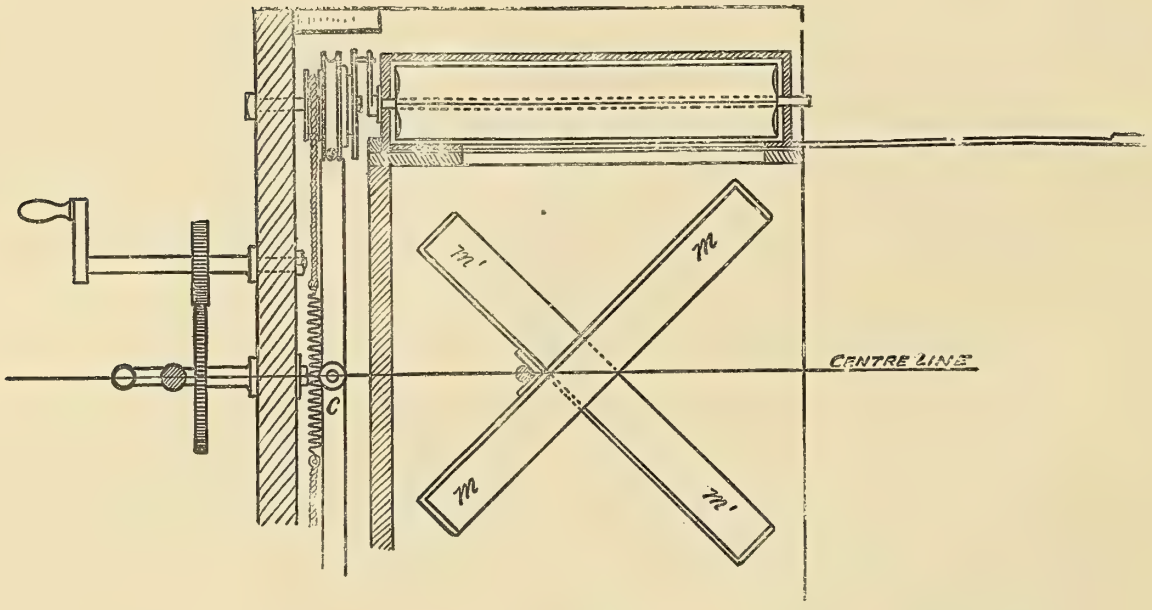

Fig. 5.

For the greater part of the excursion of the lever *l l* this arrangement has no effect on the plunger *p p*; but just at the end of its journey, the lever *l' l'* comes in contact with one or other of the stops *t t*, and this causes a quick

displacement of the plunger p in the opposite direction to that in which the lever is working.

The actual result is, that during the upward motion of the lever the right-hand drum is stationary, and the full beam of light forming the spectral image is concentrated on one of the six plates mounted on it; at the same time the left drum, on which no light is falling, is being turned into a new position, to be ready for the next exposure. Just before the end of the upward stroke the plunger p is forced quickly down, and the beam of light is transferred to the left drum, which has just been brought into position; but for a small fraction of time the spectrum is actually exposed on both plates, *i.e.* the lower plate on one and the upper on the other. As the motion of the mirrors is strictly in their own planes, this motion has no effect on the position of the images on the plate, and consequently does not, if properly adjusted, impair the definition.

IV.

Dr. A. A. Rambaut's Report.

The object of the spectroscopic observations undertaken by us was to obtain two series of spectra, at second and third contacts, with the idea of determining the order in which various lines appeared in, and faded out of, the flash and chromospheric spectra.

It was at first proposed to use a slit spectroscope, on an equatorial mounting, and to project a very narrow spectrum upon a uniformly-moving plate. With such an instrument, if the slit were kept continually tangent to the advancing limb of the Moon, a continuous record would be obtained, beginning at one end with the ordinary solar spectrum merging into the flash, and from that into the spectrum of the corona.

What we chiefly desired to test was, whether the change from the absorption spectrum to the 'flash' spectrum took place simultaneously for all the lines, or whether some became reversed earlier than others, as might be expected to occur if the absorption of different lines took place at different depths in a reversing layer.

This method would, however, have required a good equatorial mounting, and very accurate clockwork, to enable the observer to keep the slit tangential to the Moon's limb during the whole period.

For a long time it was very uncertain whether I could join the expedition, and it was not till the middle of April that I definitely decided to go. It was then too late to procure the necessary telescope, equatorially mounted; and accordingly a less complicated instrument, which could be more rapidly constructed, was substituted for it.

This instrument, designed and constructed by Sir Howard Grubb, consisted

of a prismatic camera, placed in a horizontal position, and fed by a flat mirror placed in front of the prism.

The object-glass of the telescope was of 4-in. aperture, and about 4 ft. focal length, and was corrected for the photographic rays. A dense flint prism, of about 38° refracting angle, and a circular silvered-glass mirror, of 12 inches aperture, were employed.

The peculiarity of the instrument consisted in the apparatus for obtaining a continuous succession of spectra, as described in Sir Howard Grubb's Report above.

As it was intended that the exposures should not exceed 2 or 3 secs., we considered that, with the moderate dispersion of the prism used, it would be unnecessary to apply any movement to the 12-in. mirror during the short period for which each plate was exposed.

It would have been possible, of course, to place the fixed mirror so as to reflect the image of the Sun into a horizontal telescope in any azimuth. But there was one point of the horizon which possessed an important practical advantage over any other. It was essential that the rod, or plunger, to which the mirrors were attached, should move up and down in a vertical direction, and of course the refracting edge of the prism had to be placed parallel to this rod. It was also essential that the refracting edge of the prism should be parallel to the tangent to the Sun's image at the point of contact, or as nearly so as possible. In order, therefore, that the prism should be used with its edge vertical, it was necessary to arrange the apparatus so that the diameter of the Sun's reflected image, which passed through the point of contact, should be horizontal. It was therefore necessary to find the particular position in which the camera should be placed, so that these conditions might be fulfilled.

This is found in the following way :—

Let *HAMS'* represent the horizon, *HZB* the meridian, *S* the position of the Sun, *Z* the zenith, and let *ASB* be the direction of that diameter on which the given point of contact lies. Then the angle *ZSB* (= *V*) is "the angle from the vertex," measured in the direction *NESW* which may be computed from the Nautical Almanac. Also, *ZS* is the zenith distance of the Sun (= ζ) and *BZS* is the Sun's azimuth (= *A*).

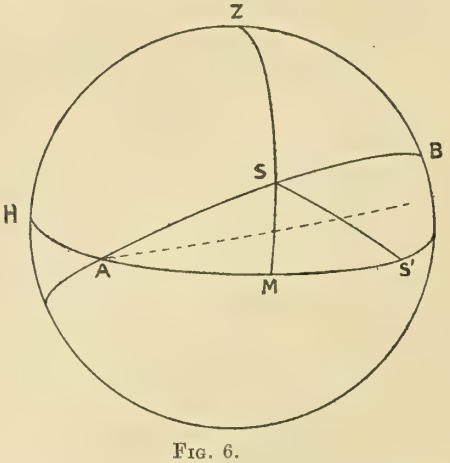

Fig. 6.

Draw the great circle *AN* bisecting the angle *SAS'*, and take *AN* equal to $\frac{\pi}{2}$; join *SN*, and produce it to meet the horizon in *S'*. Then it is clear that *S'* is the

required point, and that if a flat mirror be placed with its normal directed to N, and a telescope whose axis passes through the point S' be directed upon it, the image formed in the focus will be so placed that the diameter ASB will be reflected into a horizontal position.

The position of S' can be very easily computed from the following formulæ:—

From fig. 6 we have

$$\tan AM = \cos \zeta \tan V.$$
$$\tan AS' = -\cot \zeta \sec V.$$

Hence, we obtain

$$\tan MS' = \tan (AS' - AM) = \frac{\cos \zeta \cos V}{\sin V - \sin \zeta},$$

and the azimuth of the point S' is found by subtracting MS' from the Sun's azimuth ($= A$).

This is the direction to which the beam ought to be reflected by the mirror. We have still to allow for the deviation due to the prism. If this be denoted by Δ, we have for the azimuth in which the axis of the telescope should be laid,

$$A - MS' \pm \Delta.$$

We have also

$$\cos SAS' = \sin \zeta \sin V,$$

and

$$\cos SS' = \sin^2 AS' \cos SAS' - \cos^2 AS'$$
$$= \sin \zeta (\sin V - \sin \zeta) / (1 - \sin V \sin \zeta),$$

by the aid of which the angle of incidence ($\frac{1}{2} SS'$) of the light upon the mirror can be easily computed.

If the observing station had been situated exactly on the line of central eclipse, then the position of S' would have been practically the same for the second and third contacts, neglecting the small shift due to the motion of the Sun during the short time for which the total phase lasted; and in that case the same adjustment in azimuth would have been suitable for both phases.

Our station was, however, situated at some little distance south of the central line, and in consequence the tangents at the points of second and third contact were no longer strictly parallel. The azimuth of the reflected beam, computed for the two cases by the above formulæ, were $-30^\circ\ 7'$ and $-12^\circ\ 30'$ respectively. The difference therefore amounted to only $17^\circ\ 37'$, and on considering the risk and difficulty of making a readjustment within the few seconds available for the purpose at mid-totality, we decided to be satisfied with a position of the instrument which would be a mean between the two positions given above. The angle between the direction of the refracting edge of the prism and the tangent at the point of contact in the reflected image did not, however, even in the extreme cases (the first and last exposures), amount to as much as 9°. The effect of this is very small, and in no way detracts from the value of the photographs, while the

result proved that the freedom thus obtained from anxiety with regard to a delicate adjustment, in the excitement of mid-totality, was a most substantial gain.

As a check on the adjustment of the instrument, a small collimator was attached rigidly to the camera, with its object-glass placed immediately above the prism. Behind the eye-piece of this telescope was placed a card, with two cross-lines marked upon it. This telescope was fed with light from the 12-inch mirror, which also supplied the camera itself, and it was adjusted so that when the Sun's image was placed centrally on the cross-lines of the collimator, the spectrum fell upon the middle of the exposed photographic plate. If from any cause, either from an accidental jar, or in consequence of the diurnal motion, the spectrum became displaced upon the plate, it was possible by this device to bring it back at once to its proper position by a readjustment of the mirror. In this way the spectrum was suitably placed a few seconds before the total phase began; but during totality the displacement was not of sufficient importance to necessitate a readjustment of the mirror.

Sir Howard Grubb took charge of the exposure-cap and the flat mirror and, as the total phase approached, stood ready to readjust the latter, should the finder show it to be necessary to do so. At the same time, I stood grasping the handle, and ready to commence turning it when I thought the proper moment had arrived. We had intended to commence exposing plates 5 secs. before the beginning of totality, as nearly as possible; but, although the diminishing arc of sunlight appeared to be still of considerable width when I gave the word to uncap, I was surprised to hear, almost immediately afterwards, as it appeared, Dr. Downing's signal that the total phase had begun. The interval appeared to be about 2 secs., and certainly the first plate had been exposed before Dr. Downing, who was observing with a celluloid grating in one tube of a field-glass, saw the flash-lines, and gave the signal.

The exposures were continued without intermission till 15 secs. after the second contact, commencing with a duration of 1 sec., and gradually increasing in length, until at the end they occupied rather more than 2 secs.

When the whole twelve plates had been exposed, our business was to close the slides, to remove the two carriers, to replace them by a similar pair containing fresh plates, and to withdraw the slides of those in readiness for the exposures to be made before and after the third contact. Unfortunately, at this stage, a hitch occurred, one of the slides being overlooked. The changing of these carriers occupied us for about 25 to 30 secs., after which we had about half a minute to view the eclipse with the naked eye, and by the aid of binoculars.

At 65 secs. from the beginning of totality the exposure of the second series of plates began, at first slowly, giving about three seconds, and gradually

increasing the speed, until at the end (about 3 secs. after totality was over) the handle was turning at about the rate of one rotation per second. At this instant the increasing sunlight was so strong that, although the handle had been turned only ten times, and thus only ten exposures given, fearing lest some of the light might be reflected in the interior of the carriers, and thus fog the plates already exposed, I gave Sir Howard Grubb the signal to cap, just as the latter was about to do so on his own responsibility, as it had been arranged that he should do if it appeared to him that the exposures were being too much prolonged. We should thus, in any case, have lost the last two plates of this series; but, in addition, the oversight which had occurred in leaving one of the slides undrawn robbed us of half of the remaining ten, so that we have only five photographs relating to the latter phase of the eclipse.

The twelve plates taken near second contact give an uninterrupted record of the changes in the chromospheric spectrum during the 17 or 18 seconds over which they extend.

The instrument was focussed by means of a collimator, with a narrow slit in its focal plane. The collimator itself was focussed for parallel rays by means of an eye-piece behind the slit, placed so that the latter was exactly in its focus. The slit was then opened, and the collimator focussed as an ordinary telescope on a distant mountain peak.

In focussing the camera, the Fraunhofer lines in the green and blue were made as sharp as possible, and then an allowance made for the difference in focus between rays corresponding to this part of the spectrum and those in the neighbourhood of H and K. Unfortunately this allowance was not hit off with perfect success, and in consequence the definition of the lines is not so sharp as might be desired, especially in the lower end of the spectrum. Towards the violet end the definition is better, but the sensitiveness of the plates (Sandell's triple-coated) seems to fall off rapidly for wave-lengths beyond $\lambda 3650$.

Several of the plates are disfigured by the effect of a slight jar which took place at one part of the movement, just as the light was being cut off one plate and turned on to another on the other side of the camera, and in nearly every case the brighter lines are accompanied by a straight wing, which seems to have been due to some irregular reflection from the moving mirrors. Neither of these defects, however, although they spoil the appearance of the plates very much, can be said to detract seriously from their scientific value.

In the first two of these plates the F, H, and K lines of the chromosphere are exhibited boldly, H and K extending through an arc of nearly 180°, while F covers about 130°. The hydrogen lines, H_γ and H_δ, are the next in order of brightness, and extend through about 90°. Somewhat fainter than these are the H_ζ and H_η of hydrogen, the strontium line at $\lambda 4078$, and a line at $\lambda 4023$. Then

follow a series of short lines, some of which seem to belong to the flash layer; but whether it be due to the shortness of the exposure, or the want of sharpness in the focussing, the number of lines of the reversing layer which have impressed themselves is small. The gradual fading out of the lines as the eclipse advanced is exhibited in the analytical table (p. 84), and in fig. 1, Plate VIII.

The order in which the lines disappear is for the most part the order of their brightness in Nos. 1 and 2 of this figure, although there are one or two exceptions to this rule, as, for instance, the two lines of strontium at λ4078 and λ4216, which disappear earlier than other lines of the same original intensity. The figures given for the intensity are *eye-estimates*, on a scale of 10; but, although *as such* they cannot be relied on with absolute certainty, I think that they are probably correct to within a unit on this scale, and that a departure from the rule, such as occurs in the case of these lines, indicates a real change in the spectrum.

It will be noticed that on the last spectrum of this series, which was exposed from about 13 to 15 secs. after the beginning of totality, the only lines which remain are F, H, and K. On the first plate of the second series, taken towards the end of totality, there are four other lines, viz., λλ4472, 4341 (H_γ), 4102 (H_δ), 3889 (H_ζ), and possibly λ4078. From this it may be concluded, with some degree of confidence, that the interval between the exposure of this plate and third contact was less than that separating second contact and the exposure of plate No. 12 of the earlier series.

For this second series the rotation of the handle was commenced at the 65th second from the beginning of totality; but until the plates were developed there was no means of knowing whether, at the first exposure, the light was reflected to the right or the left of the camera, and therefore it was doubtful whether the first plate, on which a spectrum was found, corresponded to the first or second turn of the handle. If it fell to the right, then, the slide on that side not having been drawn, no corresponding spectrum would appear, and the first spectrum would correspond to the second turn of the handle. This uncertainty renders it a little doubtful at what instant each plate of this series was actually exposed.

The fact, however, that the first plate of the second series contains more detail than the last of the first series, indicates pretty conclusively that it was exposed by the second turn of the handle, and that the first turn had directed the light to the right where the plate was not exposed, or that this exposure (which lasted for about 3 secs.) extended from the 68th to the 71st second of the total phase. If this is the case, the others would correspond to the following intervals: $73^s{\cdot}5 - 75^s{\cdot}5$, $77^s - 79^s$, $81^s - 82^s{\cdot}5$, $84^s - 85^s$, counting in all cases from the beginning of totality.

The last photograph of all shows considerably more detail that any of the others, a large number of lines being found on this plate only (*cf.* Table), especially

at the extreme violet end. In this we seem to have the true flash; but the number of lines is very much inferior to what we should in all probability have obtained with a better focus.

A narrow line parallel to the length of the spectrum appears in the ultra-violet part of each spectrum. This is due to the planet Mercury, which was situated about 2° preceding, and a little south of, the Sun's centre.

In order to indicate graphically the successive changes taking place in these two series of spectra, a series of straight-line spectra have been derived from the prismatic camera negatives, and placed directly under one another, so that the gradual fading out of the lines may be seen at a glance.

To obtain these photographs a very narrow strip through the middle of each spectrum was taken (the rest being masked out), and projected in an ordinary enlarging apparatus upon a plate, which was moved to-and-fro by clockwork, in a direction at right angles to the length of the spectrum.

The spectra, thus broadened to a quarter of an inch in width, were photographed on three plates: Nos. 1 to 6 of the first series being taken on one plate, and Nos. 7 to 12 on another, while the third plate carried the five spectra of the second series. In fig. 1, Pl. VIII., the whole twelve of the first series are reproduced together, while the five of the second series are shown in fig. 2 of the same plate.

Unfortunately the motion of this clockwork was not quite uniform throughout its whole range, which has had the effect of producing a band of greater intensity throughout the length of the spectrum. This does not, however, affect the utility of the photographs. A more serious defect is the appearance of a number of very fine lines *across* the spectrum, which might, at first sight, be taken for true spectral lines, but which are really due to the coarseness of the film, each inequality being drawn out by the motion of the plate into a fine line. These mock lines can, however, be easily distinguished from the true spectral markings, partly by their fineness, and partly by the fact that the true lines are for the most part traceable through several consecutive spectra. In three cases, viz., in spectra 2, 8, and 10, a thin *dark* line may be noticed. These are due to minute pinholes in the negatives.

In this part of the work I have to acknowledge the very great help I have received from Mr. E. E. M'Clellan, third assistant at the Radcliffe Observatory, who not only developed the original negatives, but devoted a great deal of time and skill to the production of figs. 1 and 2, Pl. VIII.

The measurement of the photographs was carried out at the Radcliffe Observatory, using a microscope-micrometer, constructed in 1896 by Hilger. In this connexion I desire to express my acknowledgments to Mr. John Evershed, F.R.A.S., who was kind enough to supply me with a MS. list of chromospheric

and flash lines photographed during the Indian Eclipse of 1898, from an (as yet) unpublished memoir.* This list has been found of the utmost utility in identifying the lines.

In the following Table the first and last columns contain a current number; the second column gives the wave-length of the line; in the third is given its estimated intensity on the first photograph of the first series; while the fourth contains its intensity as estimated on the last photograph of the second series. The fifth and sixth columns give the numbers of the plates in the two series on which the line appears; the seventh column gives the corresponding element; and the eighth the usual designation of the line.

No.	Wave-length.	I_1	I_2	Plates at beginning.	Plates at end.	Element.	Designation.	No.
1	3572		0–1		5	Sc		1
2	3576		0–1		5	Sc?		2
3	3581		0–1		5	Fe		3
4	3585		0–1		5	Fe		4
5	3590		0–1		5	C?		5
6	3594		0–1		5	Fe? Cr?		6
7	3597		0–1		5	Fe?		7
8	3603		0–1		5	Cr?		8
9	3609		1		5	Fe		9
10	3614		1		5	Sc		10
11	3619		1		5	Fe		11
12	3624		1		5	Fe		12
13	3628		0–1		5	?		13
14	3630		1		5	Ca?		14
15	3641		1		5	Ti		15
16	3644		0–1		5	?		16
17	3647		0–1		5	Fe		17
18	3652		0–1		5	Fe?		18
19	3660		0–1		5	H_{31}		19
20	3663		1		5	H_{29}		20
21	3672		0–1		5	H_χ		21
22	3674		0–1		5	H_ϕ		22
23	3676		0–1		5	H_υ		23

* Since published in the Philosophical Transactions of the Royal Society, vol. 197, pp. 404–410.

No.	Wave-length.	I_1	I_2	Plates at beginning.	Plates at end.	Element.	Designation.	No.
24	3678		1		5	H_τ		24
25	3683	0–1	1	1, 2, 3?, 4, 5?	5	H_σ		25
26	3687	2	2	1–6, 7?	3–5	H_ρ		26
27	3691	0–1	1	1–5	3–5	H_π		27
28	3697	1	2	1–5	4, 5	H_o		28
29	3704	1	2		4, 5	H_ξ		29
30	3707		1	1–5	3?, 5	Ti?		30
31	3710		0–1		4, 5	Fe?		31
32	3712	1	2	1–5	3?, 4, 5	H_ν		32
33	3720		0–1		5	Fe		33
34	3722	2	2	1–6	3?, 4, 5	H_μ		34
35	3728		0–1		5	Fe?		35
36	3735	2	2–3	2, 3, 5?, 7?	3–5	H_λ		36
37	3737	1	1	1–6	5	Fe?		37
38	3744		1		5	Fe?		38
39	3747	0–1	1	2	4?, 5	Fe?		39
40	3750	2	3	1–6	3–5	H_κ		40
41	3759	3	4	1–4, 5?, 6?, 7?	3–5	Ti		41
42	3761	2	2	1–7	4, 5	Ti		42
43	3771	3	3	1–7	2?, 3–5	H_ι		43
44	3777		0–1		5	?		44
45	3786		0–1		5	?		45
46	3798	3	4	1–7	2?, 3–5	H_θ		46
47	3807	0–1	1	2	5	Fe		47
48	3820	0–1	1	1, 2	4, 5	Fe		48
49	3835·5	4	5	1–8, 9?	2–5	H_η		49
50	3838	1	2	1–6	2?, 3–5	Mg		50
51	3863	1	2	1–3	4, 5	?		51
52	3889	5	6	1–10, 11?	1–5	H_ζ		52
53	3901	1	1	1?, 2	3, 4, 5	Ti		53
54	3913·5	1	1	1?, 2, 3?	4, 5	Ti, Fe		54
55	3934	10	10	1–12	1–5	Ca	K	55
56	3944	1	1	1?, 2	5	Al		56
57	3969	10	10	1–12	1–5	Ca, H_ϵ?	H	57
58	3984		0–1		5	Fe, Cr		58

No.	Wave-length.	I_1	I_2	Plates at beginning.	Plates at end.	Element.	Designation.	No.
59	4003		0–1		5	Fe ?		59
60	4023	3	3	1–7, 8?	2–5	Fe ?		60
61	4046	1	2	1, 2, 3?, 7?	3–5	Fe		61
62	4062	0–1	2	1, 2, 3?	3–5	Fe		62
63	4078	4	4	1–6, 7?	1?, 2–5	Sr		63
64	4102	7	7	1–10, 11?	1–5	H_δ		64
65	4167	0–1	1	2, 3?	5	Fe ?		65
66	4216	3	4	1–5, 6?	3–5	Sr		66
67	4227	2	3	1–4	4, 5	Ca	g	67
68	4245	2	3	1–4	3?, 4, 5	Fe		68
69	4275	1	1	1–4	5	Cr		69
70	4290	1	1	1–4	4, 5	Ti		70
71	4341	8	8	1–11	1–5	H_γ		71
72	4375	0–1	0–1	1, 2, 3?	3?, 4?, 5	?		72
73	4395	2	2	1–4	3?, 4, 5	Ti		73
74	4415	1	1	1–3	3?, 4, 5	Fe		74
75	4443	2	2	1–4	3?, 4, 5	Ti		75
76	4472	5	5	1–10, 11?	1–5	He		76
77	4501	2	2	1–4	4, 5	Ti		77
78	4523	1	2	1–6	4, 5	Ti ?		78
79	4550	2		1–5		?		79
80	4566	1	2	1–4	4, 5	?		80
81	4586	0–1		3		Fe ?		81
82	4591		0–1		5	Ti ?		82
83	4617	0–1		3		Ti ?		83
84	4637	0–1	0–1	3	5	Fe ?		84
85	4690	1		3–6		?		85
86	4713	3	1	1–7	4, 5	He		86
87	4861·5	8	8	1–12	1–5	H_β	F	87
88	4922	3	3	1–6	4, 5	P. He ?		88
89	5017	3	3	1–6	4, 5	P. He ?		89
90	5167	3	3	1–4, 5 ?	4, 5	Mg	b^{ii}, b^{iii}, b^{iv}	90
91	5254		1		5	Fe		91
92	5390		1		5	Fe		92
93	5406		1		5	Fe		93

Temperature of the Air during the Eclipse.

With a view to determining the changes in the air temperature during the progress of the eclipse, I brought out with me a *Richard* thermograph of the usual pattern, except that the clockwork was so modified that a revolution of the drum occupied only three hours (instead of a week, as in the ordinary type), so as to get an open time-scale. This was placed in a well-ventilated wooden stand, and shielded from the direct radiation of the Sun. A good mercurial thermometer, which had previously been compared with a standard thermometer at the Radcliffe Observatory, was suspended in the same stand, with its bulb in close proximity to the tube of the thermograph. Just before the beginning of the eclipse—at $2^h\ 50^m$ G. M. T.—the mercurial thermometer was read, and the corresponding point of the thermographic record marked upon the sheet. A similar comparison was made at three subsequent epochs, with the following results:—

	G.M.T.	Reading.
At	$2^h\ 50^m$	82°·3 F.
	3 20	82 ·0 ,,
	4 20	75 ·0 ,,
	4 41	76 ·8 ,,

From these four points on the curve the time and temperatures were subsequently determined by Mr. Wickham, the first assistant at the Radcliffe Observatory, as indicated in fig. 7.

It will be seen from this diagram that the change in the temperature of the air, due to the coming on of the total phase, was not very striking, amounting to only 8°·2.

The fall in temperature, which was on the whole very steady during the partial

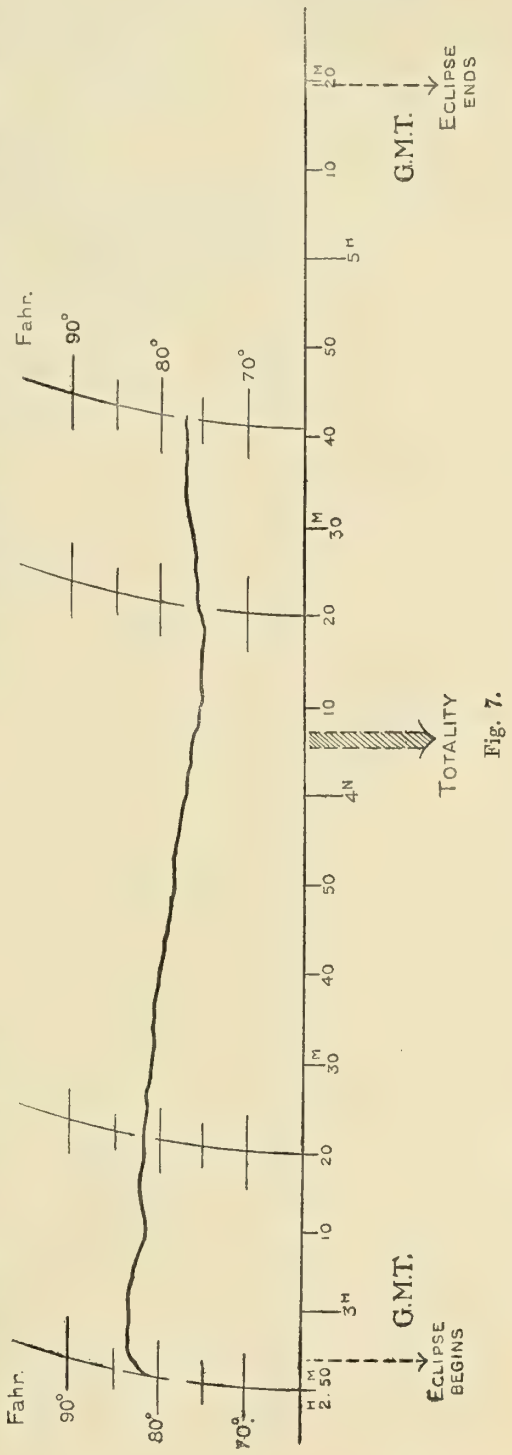

Fig. 7.

phase preceding totality, began at about $3^h\ 1^m$, or about 8^m after first contact. The minimum is not very well marked, or rather there are two minima, occurring at $4^h\ 11^m$ and $4^h\ 18^m$, respectively, between which a slight rise of about three-fourths of a degree occurs. This rise is probably due to an air-current bringing some air, previously warmed up by the direct rays of the Sun, into the thermometer screen. For though it was not specially noted that any wind arose during totality, yet such a phenomenon is a usual accompaniment of the total phase of an eclipse. The mean of the two times, given above, is $4^h\ 14{\cdot}5^m$, and, if we take this to be the time of the true minimum, we find it lagging behind the time of mid-totality, which occurred at $4^h\ 6{\cdot}2^m$, by almost the same interval as the first falling off in temperature lagged behind the beginning of the eclipse.

The rate of increase of temperature after totality appears to have been very similar to that of the decrease in the earlier phase; but the observations were discontinued at $4^h\ 41^m$, as it was found that the Sun's direct rays were just beginning to reach the thermograph through an opening in the screen, which had been insufficiently closed.

Corona, May 28, 1900.

CORONA, MAY 28, 1900.

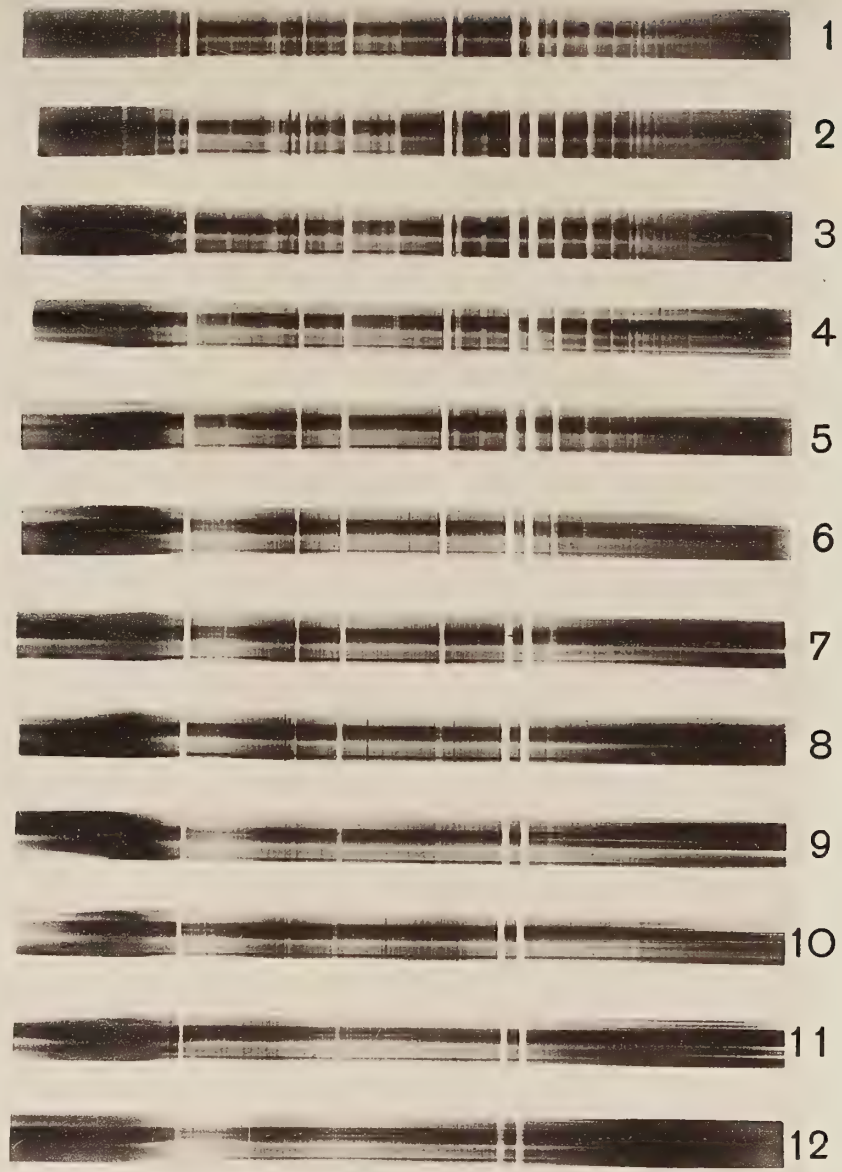

Fig. 1.

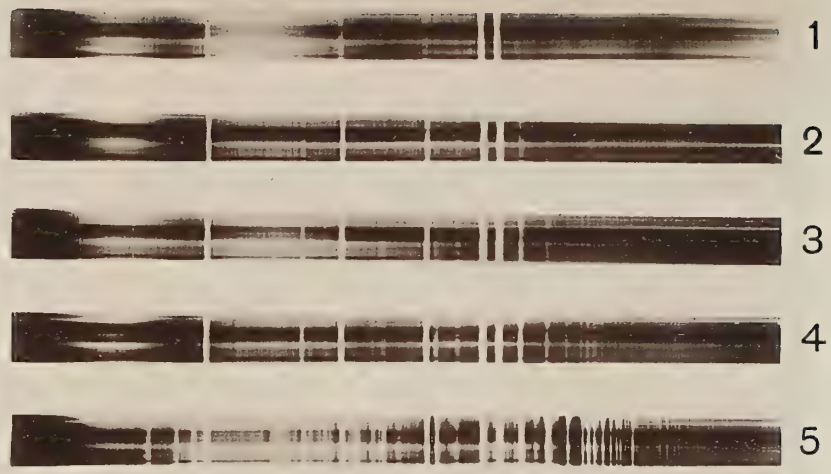

Fig. 2.

Fig. 3.

TOTAL SOLAR ECLIPSE OF 1900.

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VOLUME VIII.—(SERIES II.)

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ON THE EMANATION GIVEN OFF BY RADIUM.

BY

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Professor of Experimental Physics, University College, Dublin.

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[Read, January 19, 1904.]

The α rays of radium have been proved to consist of positively charged particles moving with great velocity, the mass of the particle being comparable with that of the hydrogen atom. The β rays have also been shown to consist of charged particles moving with great velocity, the charge in this case being negative, and the mass of the particles very small compared with that of even the hydrogen atom.

Little is known as yet about the γ rays, except that they have very great penetrating power.

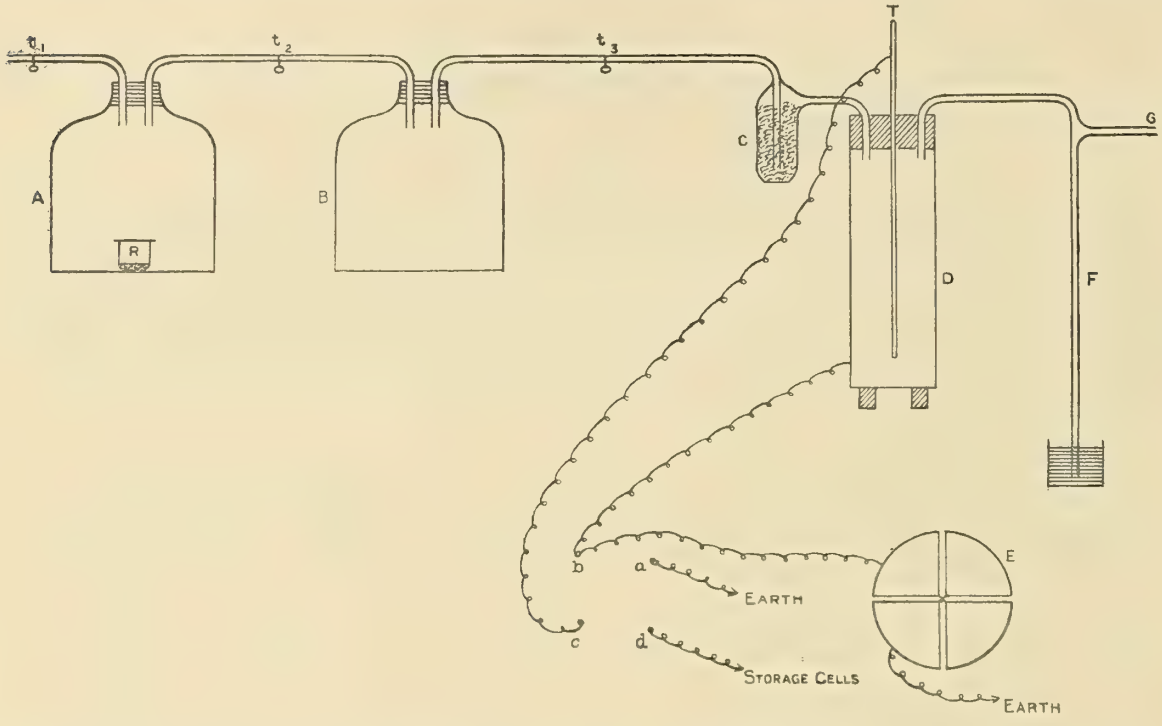
The emanation produced by radium has been much studied, and many of its properties are known; but it does not appear to have been definitely settled whether or not the emanation particles are charged; and it is important to be certain on this point when framing a conception of the manner in which the radium atom disintegrates. The object in this paper is to test as accurately as possible whether or not the emanation carries an electric charge. Rutherford's work indicates that it is not charged; but I have thought it advisable to make a direct test of the matter, as Rutherford's work is not conclusive on this point.

Rutherford has had emanation for long periods in closed vessels, and under the action of an electric field, in which case we should expect the emanation, if charged, to be driven to one or other of the terminals; and as this does not happen, the indication is that it is not charged. But if the mass travelling with the electric charge were great in comparison with the charge, the motion under electric force would be very slow, and the emanation would not move to the terminals.

Description of Apparatus.

Five milligrammes of radium bromide are dissolved in water contained in a small vessel R covered with a slip of thin paper through which the emanation readily passes. The vessel R is placed under a large air-tight bell-jar A. A second large air-tight bell-jar B is joined up as shown in the figure. C is a vessel filled with glass wool; and D is a metal cylindrical vessel resting on blocks of paraffin, and fitted with a paraffin stopper, in which is fixed the metal rod T. The glass tube F dips into mercury, and acts as a gauge to show the pressure when D is partially

exhausted, the exhaustion being produced by applying a pump at G. The letters t_1, t_2, and t_3 denote taps by which the tubes can be closed at the points indicated. E is a quadrant electrometer, one pair of quadrants being permanently to earth, and the other pair joined to a mercury cup b in a block of paraffin. The cup b is kept connected to an earthed cup a, except when an observation is to be taken,



and then the connecting piece is removed by a string from a distance, so as not to disturb the electrometer by induction effects. The mercury cups b and c in the same block of paraffin are joined to D and T; while a fourth cup d is joined to one pole of a battery of small storage-cells, the other pole of which is to earth. The vessel D is screened from outside electrical disturbances by a surrounding earthed conductor not shown in the diagram.

Method of Working.

The radium emanation passes readily through the slip of thin paper covering the vessel R; and thus, after A has remained closed for a short time, it contains a large quantity of emanation. The tap t_2 being closed, the vessels B, C, and D are partially exhausted to any desired pressure; t_3 is then closed, and t_1 and t_2 opened; and thus the vessel B is filled with air containing radium emanation, t_1 and t_2 being then closed again. The mercury cups b and c being joined together, the piece connecting a to b is removed, and the emanation allowed to rush into D by opening the tap t_3. If this emanation carries a charge, it will be shown by a

deflection of the electrometer spot of light. The glass wool in the vessel C stops dust particles which might get electrified by friction, and produce a deflection. The glass wool also stops the ions which have been produced by the radiation from the radium emanation. As the ionised gas has been for some time in B and the tube leading to C, there would be a tendency for more negative than positive ions to be lost by diffusion to the walls; and the excess of positive would produce a deflection when admitted into the vessel D.

To test whether or not the air thus admitted into D has carried emanation with it, and how much, the ionisation current between T and D is measured immediately after the gas is admitted to D. To do this c is disconnected from b, and joined to d. The terminal T is thus kept at a high potential; and the air in D being kept ionised by radiation from the emanation, the vessel D will gradually be charged; and the rate of charging is measured by the rate of movement of the spot of light when the connexion between a and b is broken.

Before the emanation is admitted there is only a very small current to D, when T is connected to the storage-battery, this small current being due to the weak ionisation which is always present in atmospheric air.

We thus, by one experiment, measure the charge (if any) carried by the emanation; and by a second experiment we measure the ionising power of this emanation.

The Observations.

We shall now give the numbers obtained in an experiment similar to a great number of others carried out.

The capacity of the electrometer and the necessary connexions, including the vessel D, was 131 electrostatic units, or ·000145 microfarad, and the electrometer gave a deflection of 60 scale-divisions for 1 volt difference of potential between its quadrants.

The admission of the radium emanation produced a deflection of only 4 scale-divisions.

The ionisation current to D was then measured as described above, immediately after the admission of the emanation. To sufficiently reduce the rate of movement of the spot of light a capacity of ·1 microfarad was joined to the electrometer, and the deflection was then 100 scale-divisions in 47 seconds.

The admission of the air containing emanation into the vessel D produced, as stated, a small deflection of 4 scale-divisions. Preliminary observations had been made to see if any deflection would be produced when an equal quantity of air, free from emanation, was admitted in the same way. It was found that a small deflection was produced probably by some friction effect, the deflection varying in different experiments between 0 and 5 scale-divisions, and being always in the

same direction. The direction of the deflection of 4 divisions obtained when the air contained emanation was the same as that obtained without emanation.

Judging not only from this particular experiment, but from several others, we are safe in saying that the emanation did not carry a charge sufficient to produce a deflection of more than 1 scale-division.

The experiment did not, therefore, detect any charge on the emanation; but it is important to calculate whether or not the emanation might be charged, and the charge be less than what could have been detected in the above experiment.

Let us suppose that each emanation particle has a charge equal to that carried by the gaseous ion; we have no case of a charge less than this, so that if the emanation is charged, its charge is probably at least equal to that of the gaseous ion, and may be greater. Denote this charge by e in electromagnetic units.

The capacity of the electrometer and connexions was ·000145 microfarad; and 1 scale-division corresponds to a potential difference between the quadrants of $\frac{1}{60}$ of a volt. A deflection of 1 scale-division would therefore be produced by putting into the vessel D a charge of

$$\frac{10^8}{60} \times \cdot000145 \times 10^{-15} = 24 \times 10^{-14} \text{ electromagnetic units,}$$

or, by admitting into D,

$$\frac{24 \times 10^{-14}}{e} \text{ emanation particles.}$$

Again, the radiation from the emanation admitted into D in the above experiment produced such an ionisation that, when T was kept at a high potential, D got a charge corresponding to 100 scale-divisions in 47 seconds, with a capacity of ·1 microfarad joined to the electrometer.

This charge is produced by ions giving up their charge to D, the charge of each ion being e.

The charge given to D per second is

$$\frac{100}{47} \times \frac{10^8}{60} \times \frac{10^{-15}}{10} = 35 \times 10^{-11} \text{ electromagnetic units.}$$

The number of ions of either sign produced in D by the radiation from the emanation is therefore

$$\frac{35 \times 10^{-11}}{e} \text{ per second.}$$

Each particle of emanation is therefore producing ions in the surrounding gas at the rate of

$$\frac{35 \times 10^{-11}}{24 \times 10^{-14}} = 1{\cdot}4 \times 10^3 \text{ per second.}$$

We see, therefore, that as the emanation, when admitted into D, did not produce a deflection of more than 1 scale-division, it must either be uncharged, or,

if charged, each particle of the emanation must give out radiation sufficient to produce at least 1400 ions per second. If the radiation from each particle were less than this, then the number required to give the observed ionisation would be greater than what would produce 1 scale-division of a deflection. This number is calculated on the assumption that the charge on the emanation is the same as the charge on the gaseous ion; it is not probable that it is less than this, if charged at all; and if it is greater, the number 1400 would be correspondingly greater.

It is, however, quite likely that each emanation particle may be capable of producing ions in the vessel *D* at the rate of 1400 per second. For this reason the test was pushed a step further.

A more sensitive electrometer was used, and the quantity of emanation was also somewhat increased. An electrometer of the Dolezalek type was employed giving a deflection equal to 4500 scale-divisions per volt difference of potential between its quadrants. With this sensitiveness, the capacity of the electrometer and connexions was 900 electrostatic units, or ·001 microfarad.

This electrometer was used to detect the charge on the emanation, and the ionisation in the vessel *D*, after the emanation is admitted, was measured by the electrometer previously used. The small deflection produced when air free from emanation was admitted into *D*, was made as small as possible before the sensitive electrometer was used; and it was finally got rid of to such an extent that the deflection was never greater than 10 divisions, varying in different experiments between 2 or 3 and 10 divisions, and being always in the same direction.

We shall give numbers observed in one experiment, using the sensitive apparatus.

The deflection on the Dolezalek was 10 divisions when the emanation was admitted. The other electrometer was then used to measure the ionisation, and gave 100 divisions in 77 seconds, with a capacity of ·5 microfarad joined to it, the sensitiveness being the same as before, 60 divisions for 1 volt difference between its quadrants.

From this experiment and several similar ones, we are safe in saying that in this case not more than 4 divisions of a deflection are produced by the emanation. It is difficult to be certain of a smaller deflection, the spot of light not being so steady as with a less sensitive instrument.

If we make a calculation of the same nature as before, we find that either the emanation is uncharged or else each emanation particle must be producing, by its radiation, at least 12,000 ions per second.

Even this radiation might be looked upon as quite possible, so that the question whether the emanation is charged or not, would not be settled. We have, however, good reasons for believing that only a small fraction of the total emanation particles are, at any instant, acting as centres of radiation and ionisation. The ionising power of emanation contained in a

closed vessel falls off with time in a geometrical progression, showing that the rate of decay of the ionising power is proportional to the ionising power at every instant—a result which readily admits of the interpretation that the radiation arises from the emanation particles undergoing some change, and that the number changing at any instant is proportional to the total number present.

The ionising power I may, from experiment (Rutherford, Phil. Mag., April, 1903), be represented by

$$I = I_0 E^{-\lambda t},$$

where λ is a constant, and t the time measured from the instant when $I = I_0$.

Since

$$-\frac{dI}{dt} = \lambda I,$$

we see that λ is the fraction of the total emanation that undergoes change or emits radiation in one second. And we know (Rutherford, Phil. Mag., April, 1903) that I falls to half its value in about 4 days, so that λ is approximately equal to

$$2 \times 10^{-6}.$$

If, therefore, we accept the theory that the emanation undergoes a further change, and that each particle acts as a centre of radiation and ionisation only when undergoing change—and this is the only theory that seems to fit in with experiment—we see that the number calculated above, giving the minimum ionisation that must be produced by each emanation particle in one second, assuming it to be charged, would have to be multiplied by the factor $\frac{1}{2} \cdot 10^6$.

Multiplying 12000 by $\frac{1}{2} \cdot 10^6$, we get 6×10^9 as the minimum number of ions produced in one second by each emanation particle when its turn comes to disintegrate, assuming that it is charged. This number is not a possible one for several reasons. Rutherford (Phil. Mag., May, 1903) gives 10^5 as the probable number of ions produced by each α ray before it is absorbed by the gas. The ionisation is chiefly due to α rays, so that to produce the above ionisation each emanation particle would require to emit

$$\frac{6 \times 10^9}{10^5}, \qquad \text{or } 6 \times 10^4 \ \alpha \text{ rays.}$$

The mass of the α particle being of the same order as that of the hydrogen atom, and the emanation having been produced by a disintegration of the radium atom, each emanation particle could not possibly emit more than about 200 α rays.

We can, therefore, finally conclude that the emanation is not charged.

This fact—that the emanation is uncharged—has an important bearing on our conception of the manner in which the radium atom breaks up. The radium atom certainly gives off positively charged particles—the α rays. The emanation particles cannot be what remains of the atom after the emission of one or more α rays, because, in that case, it would be negatively charged. The atom must have parted with an equal negative charge, either by the emission of negative particles, or in some other way.

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[Read, January 19, 1904.]

The following paper deals with the simplest case of the problem stated in the title—the case of direct reflection at the surface of a perfect conductor bounded by a plane face, and moving in any variable manner without rotation. It will first be necessary to obtain the conditions to be satisfied at the boundary. The following method of deducing them is founded on the method used by Macdonald* for the case of conductors at rest. Take axes of x and y in the tangent plane of the surface, and the axis of z normal; and let the origin of coordinates which moves with the surface have velocities in these directions u, v, w respectively. Conceive that the current (I_1, I_2, I_3), in place of being confined to the surface, takes place in a shell of very small thickness, and has a volume distribution (i_1, i_2, i_3). The current includes both conduction and convection currents which are to be estimated along axes fixed in space which for the moment coincide with the moving axes; also we suppose that essential magnetic singularities are absent. The fundamental equations now become within this thin shell—

$$V^{-2}\left[\frac{\partial X}{\partial t} - u\frac{\partial X}{\partial x} - v\frac{\partial X}{\partial y} - w\frac{\partial X}{\partial z}\right] + 4\pi i_1 = \frac{\partial \gamma}{\partial y} - \frac{\partial \beta}{\partial z},$$

$$V^{-2}\left[\frac{\partial Y}{\partial t} - u\frac{\partial Y}{\partial x} - v\frac{\partial Y}{\partial y} - w\frac{\partial Y}{\partial z}\right] + 4\pi i_2 = \frac{\partial \alpha}{\partial z} - \frac{\partial \gamma}{\partial x},$$

*H. M. Macdonald, "Electric Waves," Cambridge, 1900, p. 12.

$$V^{-2}\left[\frac{\partial Z}{\partial t} - u\frac{\partial Z}{\partial x} - v\frac{\partial Z}{\partial y} - w\frac{\partial Z}{\partial z}\right] + 4\pi i_3 = \frac{\partial \beta}{\partial x} - \frac{\partial \alpha}{\partial y}.$$

$$-\frac{\partial \alpha}{\partial t} + u\frac{\partial \alpha}{\partial x} + v\frac{\partial \alpha}{\partial y} + w\frac{\partial \alpha}{\partial z} = \frac{\partial Z}{\partial y} - \frac{\partial Y}{\partial z},$$

$$-\frac{\partial \beta}{\partial t} + u\frac{\partial \beta}{\partial x} + v\frac{\partial \beta}{\partial y} + w\frac{\partial \beta}{\partial z} = \frac{\partial X}{\partial z} - \frac{\partial Z}{\partial x},$$

$$-\frac{\partial \gamma}{\partial t} + u\frac{\partial \gamma}{\partial x} + v\frac{\partial \gamma}{\partial y} + w\frac{\partial \gamma}{\partial z} = \frac{\partial Y}{\partial x} - \frac{\partial X}{\partial y}.$$

Let us now integrate these six equations with respect to z throughout the thickness of the shell. $\int u\frac{\partial \gamma}{\partial x}\partial z$, for example, will vanish, since the quantity under the sign of integration remains finite, whilst the range of integration is very small; also we have $\int i_1 dz = I_1$, &c. Of the other quantities, only those which contain a differentiation with respect to z will not vanish. Then, since the forces vanish within the surface, we have for their values *on the surface*

$$-wV^{-2}X + 4\pi I_1 = -\beta,$$
$$-wV^{-2}Y + 4\pi I_2 = \alpha,$$
$$-wV^{-2}Z + 4\pi I_3 = 0,$$
$$w\alpha = -Y,$$
$$w\beta = X,$$
$$\gamma = 0.$$

The first three of these equations determine the currents which screen the action of the external varying field. The second three are the necessary boundary conditions.

Take axes of x and y in the face of the conductor, and the axes of z pointing into the dielectric medium. Let the distance of the origin from a fixed plane parallel to the face of the conductor be denoted by

$$h\,(=f(t)).$$

Let the incident wave-train be given by

$$X = F(Vt + z)$$

$$\beta = -\frac{1}{V} F(Vt + z);$$

and assume for the reflected system

$$X' = \phi(Vt - z),$$

$$\beta' = \frac{1}{V} \phi(Vt - z),$$

then it is required to determine ϕ.

The boundary condition is

$$X + X' = h(\beta + \beta')$$

when $z = h$, *i.e.*

$$\phi[Vt - f(t)] = -\frac{V + f'(t)}{V - f'(t)} F(Vt + f(t)).$$

By putting $Vt - f(t) = \xi$, and eliminating t, we get the value $\phi(\xi)$. For instance, if $f(t) = vt$ where v is a constant,

$$\phi(\xi) = -\frac{V + v}{V - v} F\left(\frac{V + v}{V - v} \xi\right),$$

indicating a change of amplitude and a Döppler change of period. In certain other cases—for instance, in the case of uniform acceleration—we may perform the elimination algebraically; but in all cases we can do it graphically. If we introduce functions ϕ_1 and F_1, such that

$$\phi_1'(\xi) = \phi(\xi); \; F_1'(\xi) = F(\xi),$$

then it will be sufficient to determine ϕ_1 such that

$$\phi_1[Vt - f(t)] = -F_1[Vt + f(t)].$$

For example, if $f(t) = a \sin pt$,

then

$$\frac{\xi}{V} = t - \frac{a}{V} \sin pt,$$

so that

$$t = \xi + A_n \sin \frac{np\xi}{V},$$

where

$$A_n = \frac{2}{np} J_n\left(\frac{pa}{V}\right);$$

and substituting for t in $-F[Vt + f(t)]$, we get the value of $\phi_1(\xi)$.

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There has been some difficulty in deciding as to the nature of the very penetrating or γ rays given by radium, whether they are electromagnetic pulses like Röntgen rays, or a flight of charged particles like the α and β rays. In some respects, the γ rays act like the α and β rays, which we know to consist of charged particles; while, on the other hand, no deflection of the rays in a magnetic field has been detected; and, further, we should expect to get Röntgen rays from a body like radium which is emitting charged particles travelling with a great velocity.

The chief reason for considering the γ rays to consist of charged particles lies in the fact that the absorption of these rays by different substances is proportional to the density of the substance—a law obeyed by α and β rays, and by Cathode rays, but not obeyed by Röntgen rays. It should be stated here that since the experiments described in this Paper were commenced, Mr. Eve* has found that, when only very penetrating Röntgen rays are used, the absorption produced by different substances becomes more nearly proportional to the density of the substance—a result which tends to remove the chief difficulty in deciding that the γ rays are of the same nature as Röntgen rays.

The method I have used to decide whether or not the γ rays are charged particles is to endeavour to directly detect the charge carried by the rays, if any.

First Experiment.

A block of lead was taken, and a hole drilled to a small depth in the centre of one of its faces; 50 milligrams of a good sample of radium bromide, enclosed in an air-tight vessel with a very thin mica top, were placed in the hole. The rays from

* *Nature*, March 10th, 1904.

the radium could thus escape upwards from the hole, the block being so large that no α and β rays, and very few γ rays, could escape in any other direction. This lead block was carefully insulated on paraffin, screened by an earthed conductor from outside electrical forces, and joined to a sensitive Dolezalek electrometer. The electrometer was first connected to earth, then insulated, and the charge it gradually got was observed. Observations were made—(1) when the hole in the lead block was covered with only a very thin sheet of foil, so that α, β, and γ rays could escape; (2) when the hole was covered with such thickness of foil that only β and γ rays could escape; (3) when the hole was so covered that only γ rays escaped.

It is obvious that care must be taken in screening the block of lead with an earthed conductor to guard against effects due to contact electric forces. The air between the lead block and the surrounding screen being ionised, a current will exist between the block and the screen if they are not of the same metal; and this will produce a deflection of the electrometer when insulated. This was avoided by wrapping the block and the wire leading to it with tinfoil, and making the surrounding screen also of tinfoil.

When the effects of contact electric forces are eliminated, the effects observed are as follows:—

(1) When α, β, and γ rays are allowed to escape, the lead block gets a negative charge.

(2) When β and γ rays escape, the block gets a positive charge.

(3) When only γ rays escape, the block gets a negative charge.

The results in cases (1) and (2) are what we should expect. The α rays carry away positive electricity, and the β rays carry away negative electricity. The β rays are only produced in the final stages of disintegration, so that we should expect the negative electricity carried away by the β rays to be less than the positive carried away by the α rays. We thus get a negative charge on the block when both α and β rays escape, but a positive charge when only β rays escape. The result obtained when only γ rays escape appeared at first sight to show that these rays carried a positive charge. This, however, is not necessarily so. It was found that, when the radium was taken out of the block and placed outside the screen, so that the air surrounding the block was still ionised, the lead block got a negative charge. This is due to the known result that a conductor exposed to ionised air gets a small negative charge, owing to the fact that the negative carriers travel with a slightly greater velocity than the positive carriers under the same electric force. The ionised air immediately

surrounding the lead block gives up to the block more negative than positive carriers, the block thus getting a negative charge, and the surrounding gas retaining an excess of positive.

It was easy to show that all the negative charge observed when only γ rays escaped could be accounted for in this way. The experiment was therefore somewhat inconclusive in its present form as regards the γ rays; it is described chiefly because it affords a simple method of demonstrating the charge carried by the α and β rays.

One precaution is necessary in showing the charge on the α rays in this way. These rays are very easily absorbed by the air; and thus at a small distance above the block there will be an excess of positive electricity producing an electric field tending to drive positive ions to the lead block. This can be got over by placing an earthed sheet of foil a short distance above the block. When this is done, and the effects of contact electric force eliminated, the successive changes of sign of the charges got by the block containing the radium occur as described above. The charge in each case, of course, reaches a maximum when the rate of gain of charge is equal to the rate of loss through the surrounding conducting air.

It is scarcely necessary to give any numbers, as they are not required for any calculations in this Paper. In showing the negative charge carried by the β rays, initial deflections of 100 scale-divisions in 10 seconds could easily be obtained with a Dolezalek electrometer. For the α rays, it was necessary, for the reason given above, to place an earthed plate at a short distance above the block containing the radium. The conducting gas between the plate and the block prevented the block from charging rapidly; but an initial negative deflection of 50 divisions in 15 seconds could be obtained. The numbers are merely given to show how this simple arrangement demonstrates the charge on the α and β rays.

Second Experiment.

The experiment described above is not conclusive as to whether the γ rays carry an electric charge, because of the effect due to the surrounding ionised gas. This effect could of course be largely eliminated by carrying out the experiment in a high vacuum. It was more convenient, however, to modify the apparatus in another way.

A tin cylinder b, 20 cms. long and 8 cms. diameter, was placed inside a second tin cylinder a, 25·5 cms. long and 10 cms. diameter, and the annular space between flooded with liquid paraffin, which solidified, insulating the inner cylinder from the outer, which was to earth. The inner cylinder was

filled with shot, so that it absorbed a pencil of γ rays passed along it. The 50 milligrams of radium bromide producing the γ rays were contained in a small vessel R placed in a hole in a block of lead, so as to limit its radiation to a diverging pencil with its axis parallel to the axis of the shot cylinder, and thus prevent the rays from falling on other parts of the apparatus.

A wire from the inner cylinder leads to the electrometer placed some yards away, the wire being led along the axis of an earthed metal tube filled with paraffin. The inner cylinder and the wire leading from it were thus not exposed to air ionised by the radium.

The vessel a was thick enough to stop all the α rays; the β rays could be stopped when desired by placing a sheet of lead between the radium and the vessel a.

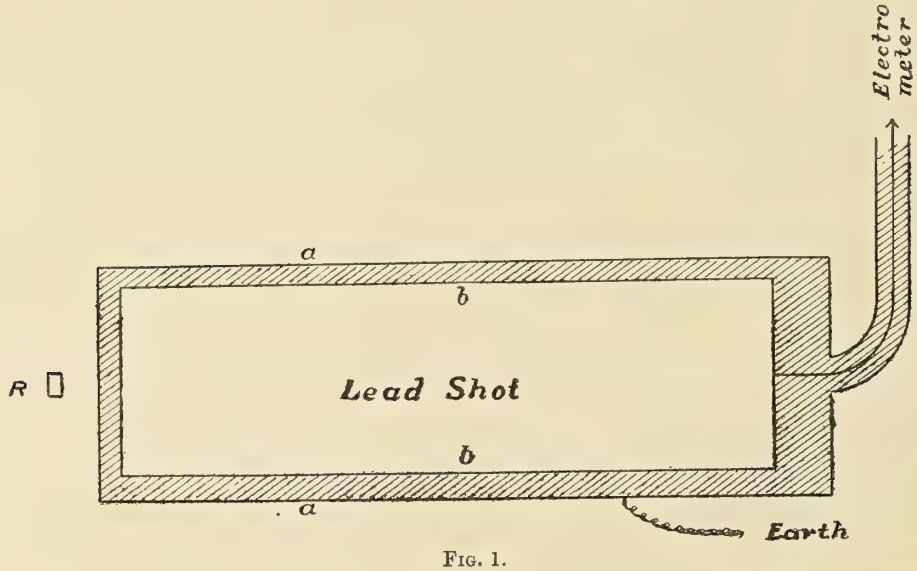


Fig. 1.

In this way the charge got by the shot-cylinder could be measured when it is absorbing both β and γ rays, and also when absorbing γ rays only.

When the shot-cylinder absorbs both β and γ rays, it rapidly gets a negative charge; when it absorbs only γ rays, no charge could be detected..

This experiment shows that the γ rays do not carry any charge sufficient to be detected by the above apparatus.

The sensitiveness of the apparatus, and therefore the value of the experiment, can best be estimated by considering the deflection obtained with the β rays. A pencil of rays diverges from R wide enough to include practically the whole end of the cylinder b. This pencil produces a deflection of 38 scale-divisions per minute when absorbed by the shot-cylinder. The whole charge carried by

the β rays contained in this pencil does not reach the shot-cylinder, part being absorbed by the cylinder a, and by the mica vessel containing the radium. The tin vessel a was ·3 mm. thick, and this thickness of tin (neglecting the paraffin and mica) would absorb four-fifths of the β rays (Strutt, *Nature*, 1900, p. 539). The β rays contained in the pencil considered, therefore, carry a negative charge corresponding to more than 200 scale-divisions per minute.

The same pencil of γ rays is experimented with, and gives no deflection—certainly not as much as two divisions per minute; the pencil in this case is not all absorbed by the shot, because of the great penetrating power of these rays. We know the coefficient of absorption of lead for these rays (fig. 4); and it is easy to calculate from the dimensions of the apparatus given that more than one-third of the γ rays included in the pencil considered would be absorbed by the cylinder of shot; and we have seen that the deflection produced is not more than two divisions per minute. We have, therefore, the result that the β rays in a given pencil carry a negative charge corresponding to more than 200 divisions per minute, while the γ rays contained in the same pencil do not carry a charge corresponding on the same scale to more than 5 divisions, positive or negative, per minute.

The absorption of β rays by the mica vessel and the insulating paraffin has been neglected in this estimate; and this absorption would be considerable, so that the electricity, if any, carried by the γ rays coming from a sample of radium is certainly not more than one or two per cent. of the negative electricity carried by the β rays from the same source. The γ rays might, of course, consist of a stream of uncharged particles; but, as the forces causing disintegration are probably of an electrical nature, this is not likely.

Absorption of γ Rays by Different Substances.

Rutherford has measured the absorption of these rays by different substances, and shown that the absorption is roughly proportioned to the density of the substance. The absorption, however, increases more rapidly than the density, so that the ratio of the coefficient of absorption to the density is greater for the denser substances, being for lead about twice what it is for water.

On account of the importance of this law of absorption, I have made some further experiments which show that when the rays have passed through some thickness of the denser substances, the coefficient of absorption is diminished, so that the law that the coefficient of absorption is proportional to the density is then followed with remarkable closeness.

Apparatus Used.

The brass tube *a*, fig. 2, is insulated, and fitted with a paraffin cork *c*, through which passes the brass rod *b*, joined by a wire to an electrometer. This wire is led along an earthed metal tube filled with paraffin. The other end of the tube *a* is closed with a very thin metal cap.

The radium is contained in a small vessel *R*, which is placed in a block of lead, so that a diverging pencil of rays travels along the tube *a*, ionising the air between *a* and *b*.

The tube *a* is kept at a high potential by joining it to a battery of small storage cells. The current through the ionised gas is measured by the electrometer, the tube *a* being kept at a sufficient potential to produce the saturation current.

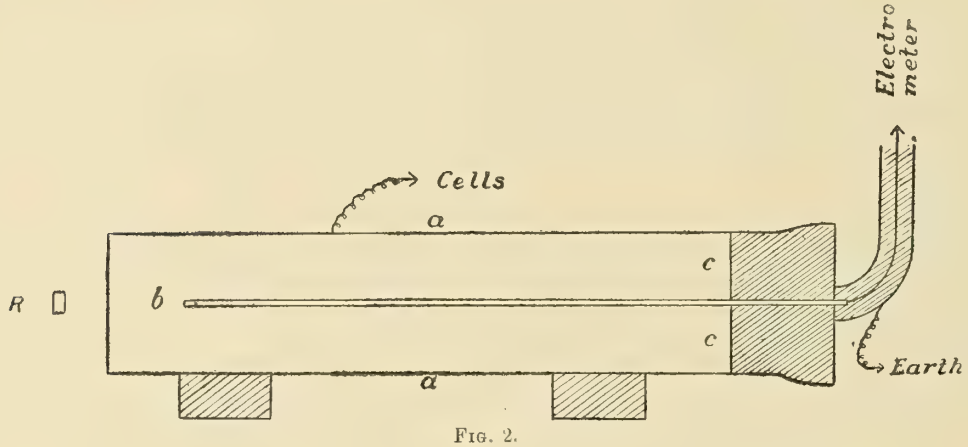


Fig. 2.

Sufficient thickness of lead is placed permanently between *R* and the tube *a* to absorb all the β rays, so that the ionisation observed is due entirely to γ rays, and measures the intensity of these rays. The substance to be examined is placed in successive layers in front of the tube *a*. In this way curves are drawn showing the intensity of the γ rays after passing through various thicknesses of different substances.

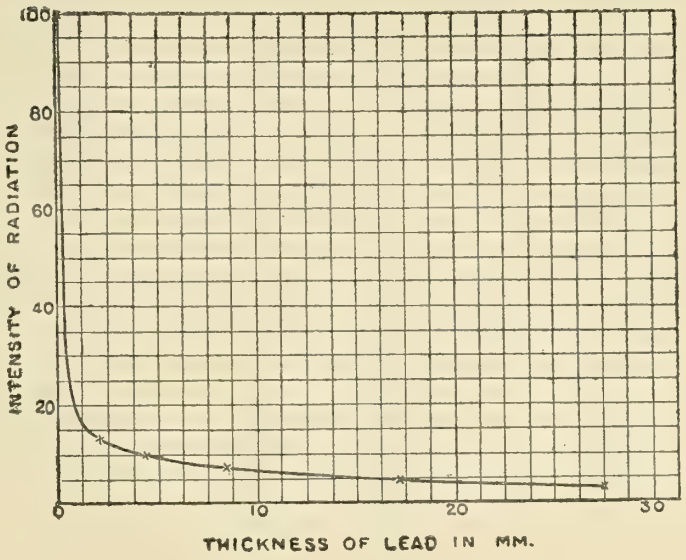
Fifty milligrams of pure radium bromide were used, placed 10 cm. from the testing tube *a*.

Observations.

In the first place, the thickness of lead required to stop all β rays was determined by observing the ionisation produced in the tube *a*, with nothing between the radium vessel *R* and the tube, and then putting successive layers of lead between; the vessel *R* itself stops most of the α rays. Fig. 3 shows that all the β

rays are stopped by 4 mm. of lead. In all the following experiments a thickness of 8 mm. of lead was kept in position between the radium and the testing tube, a thickness which ensured that all β rays were stopped.

Fig. 4 shows the result of experiments with a number of substances, the numbers along the horizontal axis expressing the thickness of the substance traversed in mms., and the numbers along the vertical axis denoting the corresponding intensity of the γ rays in arbitrary units.



IG. 3.

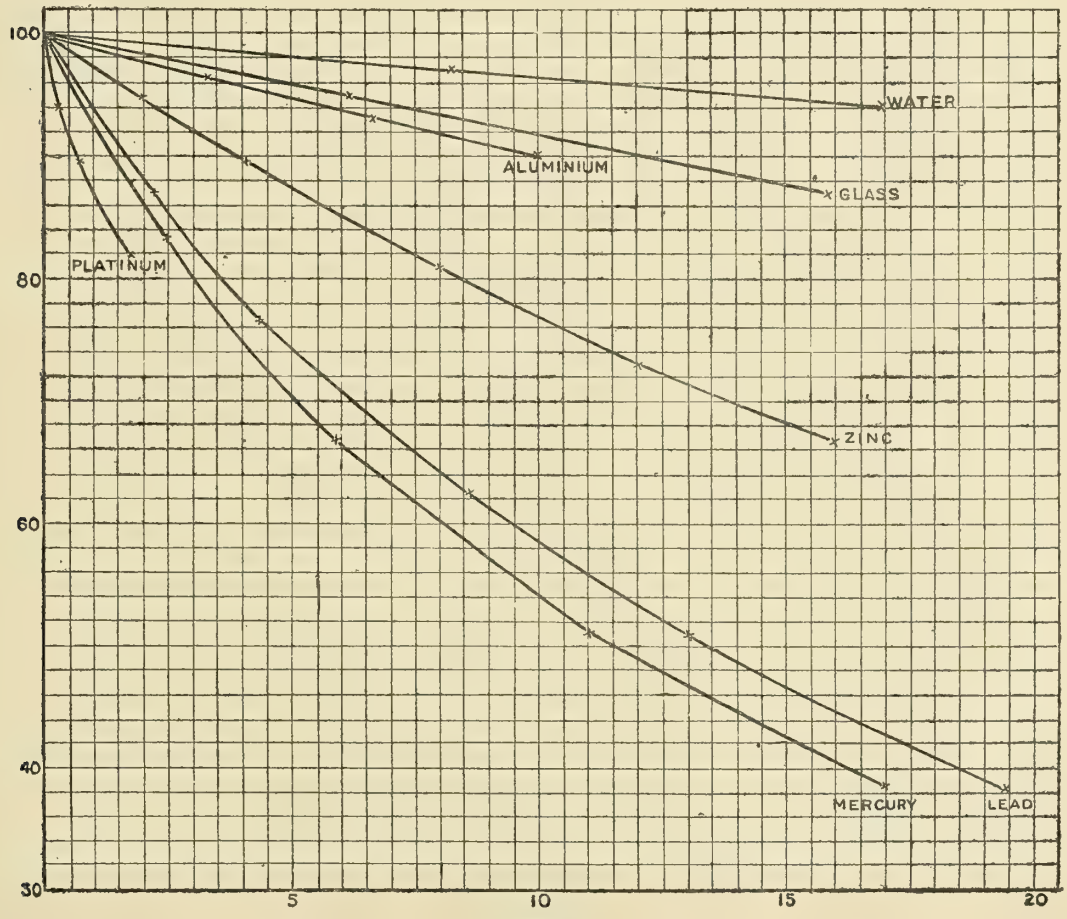


FIG. 4.

The form of the curves is not accurately given by the equation $I = I_0\epsilon^{-\lambda x}$,

I being the intensity of the rays after passing through thickness x, I_0 the initial intensity, and λ the coefficient of absorption. To represent the curves by an equation of this type, we must suppose λ to diminish with the thickness of substance traversed. These remarks apply chiefly to the curves for zinc, lead, mercury, and platinum. The absorption in the case of water, glass, and aluminium is so small that sufficient accuracy to show any change in the coefficient of absorption along the curves could not be claimed.

The coefficient of absorption has been calculated for each substance:—I., for the first 2·5 mm. of substance traversed; II., for the thickness 2·5 to 5 mm.; III., for the part 5 to 10 mm.; IV., 10 to 15 mm. The results are shown in Table A.

In this Table, for the reason given above, no attempt is made to detect any change in the coefficient for water, glass, and aluminium. It will be observed

Table A.

	I.	II.	III.	IV.
Platinum, ..	1·167	—	—	—
Mercury, ..	·726	·661	·538	·493
Lead, ...	·641	·563	·480	·440
Zinc, ...	·282	·266	·248	·236
Aluminium, ..	·104	·104	·104	—
Glass, ...	·087	·087	·087	·087
Water, ...	·034	·034	·034	·034

that for the heavier substances there is a rapid decrease of the coefficient of absorption as greater distances of the substance are traversed by the rays; no such marked change takes place for the lighter substances.

This points to the conclusion that the γ rays are somewhat heterogeneous, and that the difference between the coefficients of absorption for the more penetrating and the less penetrating parts is more marked the greater the density of the substance used to absorb the rays.

Table B gives the result of dividing the coefficient of absorption by the density of the substance. It will be seen that in column I. the numbers vary considerably, the law that the coefficient of absorption is proportional to the density not being followed at all closely. The agreement is, however, much closer in succeeding columns until in column IV. it is very close indeed. For the less penetrating radiation, the absorption increases more rapidly than the density;

but this divergence from the law disappears in the case of more penetrating radiation.

Table B.

	I.	II.	III.	IV.
Platinum, ..	·054	—	—	—
Mercury, ..	·053	·048	·039	·036
Lead, ...	·056	·049	·042	·037
Zinc, ...	·039	·037	·034	·033
Aluminium, ..	·038	·038	·038	—
Glass, ...	·034	·034	·034	·034
Water, ...	·034	·034	·034	·034

This is similar to what Eve (*Nature*, March 10th, 1904) has found for Röntgen rays. The ratio of the coefficient of absorption to the density varies between very wide limits when rays from a moderately weak tube are used, the ratio being greater for the denser substances. Eve finds that, for gases at any rate, this ratio is more nearly a constant when more penetrating Röntgen rays are used.

Rutherford has determined the coefficient of absorption of γ rays for various substances (*Nature*, p. 318, 1902). His numbers are given for comparison for substances which occur in the above Table :—

Substance.	λ.	$\frac{\lambda}{\text{Density}}$.
Water, ...	·033	·033
Glass, ...	·086	·035
Zinc, ...	·280	·036
Lead, ...	·770	·068
Mercury, ...	·920	·068

The numbers for water and glass are almost identical with those in the above tables; for lead and mercury they are greater even than those in Column I.

As mentioned at the beginning of the Paper, the fact that the absorption of γ rays is proportional to the density of the absorbing substance was in favour of these rays being charged particles, like other radiation which follows approximately the same law. We cannot, however, base much on this, as, according to Eve's work, the same law probably holds for penetrating Röntgen rays; and, apart from their absorption, the weight of evidence is certainly in favour of the γ rays being similar to Röntgen rays. We have rather to consider why charged particles travelling with great velocity and Röntgen rays should follow the same law of absorption.

Prof. Thomson* has pointed out the significance of this law of absorption in the case of charged particles. If the particle be small compared with the molecule of the absorbing substance, we must consider collisions as taking place, not with the molecule, as a whole, but with the constituents of which it is composed. If, then, all substances are built up of the same primordial atoms, the absorption by different substances should be proportional to the number of such primordial atoms per unit volume, or simply proportional to the density. Thomson also shows that the greater the velocity of the particle—the more penetrating the rays—the more closely will the absorption density law be obeyed.

It is not at first sight so clear why such a law of absorption should hold for Röntgen rays. If we regard the front of a Röntgen pulse (or a wave of light) as uniform and free from structure of any sort, it would seem difficult to explain such a law. If, however, we regard a Röntgen pulse as possessing a structure (Thomson, "Electricity and Matter," p. 63), so that the front of a pulse is not uniform, but that, at some places, the forces are great, while larger regions are free from force, we can at once get an explanation of our results.

Such a conception has been theoretically deduced by Thomson, and used to explain why, when Röntgen rays pass through a gas, some molecules are ionized, while far more are unaffected.

Applying this idea to the case we are considering, we see that if the regions of great force in the pulse are sufficiently small, we must consider their collision with the constituents of the molecule rather than with the molecule as a whole. If, in addition, the pulse is sufficiently thin—that is, sufficiently penetrating—we should deduce the same law of absorption for the Röntgen rays as for charged particles travelling with great velocity.

That a similar result does not hold for ordinary light waves is because this last condition is not satisfied.

Note added during Publication.

Two papers have recently appeared by Paschen (*Annalen der Physik*, Nos. 6 and 7, 1904), in which the author claims to show that the γ rays carry a negative charge. In the first paper the radium is surrounded with lead 1·9 cms. thick, and carefully insulated in a vacuum, when the lead gets a positive charge. It should be pointed out that under the action of Röntgen rays (according to Curie and Sagnac, Dorn and Thomson) a metal gives out negatively-charged corpuscles, thus itself getting a positive charge. The effect observed by Paschen is thus quite in agreement with the γ rays being of the nature of Röntgen rays. In the second paper the author claims to show the existence of negatively-charged radium rays, which are only slightly deflectable in a magnetic field, but this paper gives no direct proof of the γ rays carrying a negative charge.

* "Conduction of Electricity through Gases," p. 312.

TRANSACTIONS (SERIES II.).

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Vol. III.—Parts 1–14.—September, 1883, to November, 1887.

Vol. IV.—Parts 1–14.—April, 1888, to November, 1892.

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Vol. VI.—Parts 1–16.—February, 1896, to August, 1898.

Vol. VII.—Parts 1–16.—August, 1898, to September, 1902.

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[SEPTEMBER, 1904.]

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ERRATA.

Page 115, line 6 from top, *for* "impurity", *read* "element".

,, 122, ,, 5 from bottom, *delete* "Mr. Hadfield wanted", and after "the specimens", *add* the words "were required for tests".

IX.

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[Read, March 15, 1904.]

PART IV.

Thermal Conductivity.

The series of alloys that forms the subject of these researches was, as described in Part I., prepared and carefully analysed at the Hecla Steel Works, Sheffield, of which one of the authors is Managing Director. Parts I. to III., published in the Scientific Transactions of the Royal Dublin Society, dealt with the magnetic properties and electric conductivity of these alloys. We have shown that iron, when alloyed with a comparatively small quantity of certain metals, can be rendered nearly, and in some cases wholly, non-magnetic, though the iron present may amount to from 70 to 88 per cent. On the other hand, we have found certain non-magnetic elements, such as aluminium and silicon, when alloyed in small percentages with iron, render the alloy more magnetic—more permeable to magnetic lines of force at ordinary temperatures—than the purest commercial iron. The effect on electric conductivity was no less marked. A comparatively good conductor like aluminium, when alloyed, even in small quantities, with iron, enormously reduced the conductivity. In fact, the highest electric resistivity of any known metallic wire was found in some of these composite iron alloys.*

A summary of our results was given in a paper read by one of us before the Institute of Electrical Engineers;† and in that paper it was stated that "a series of experiments are in progress in which the thermal conductivities of these alloys are being measured."

The first part of the present paper deals with this aspect of the inquiry, and gives the results of a series of preliminary determinations of the relative thermal

* Sci. Trans. Roy. Dub. Soc., vol. vii., part iv.; and vol. viii., part i. (1902).

† See Proc. Institute Electrical Engineers, 1902, vol. xxxi., part 156.

conductivity of some forty of these alloys. The difficulty of absolute determination of thermal conductivity is well known, and various methods have been adopted by different experimenters.* What we have to find is the *temperature gradient*, or fall of temperature in degrees per unit length along the lines of flow of heat. The rate of transmission of heat by conduction, or the quantity of heat transferred in unit time through unit area of the cross-section of the substance perpendicular to the lines of flow, is proportional to the temperature gradient. The thermal conductivity of the substance is the constant ratio of the rate of transmission to the temperature gradient. If the flow of heat be steady, that is, if a constant difference of temperature be maintained for a sufficiently long time between two parts of the substance, the rate of transmission—in isotropic homogeneous solids—is everywhere the same, and the conductivity can thus be determined.

The difficulties in the exact determination of this temperature gradient are increased by the varying emissivity of different specimens. This is affected by so many circumstances that it forms the greatest source of uncertainty in experimental investigations. By making the substance under experiment of uniform size and shape, and uniformly silvered or blackened, these difficulties are partly overcome. But it is impossible in practice to realize exactly the theoretical conditions required for an absolute determination of thermal conductivity. Hence even the most recent determinations of the absolute thermal conductivity of the same metal vary considerably. The well-known classical experiments of Forbes on the thermal conductivity of iron, when repeated by Tait, and subsequently by Mitchell, with additional precautions, differ by from 10 to 26 per cent. in their respective values, though identically the same massive bar of iron was used, and the same method of experiment employed.

In determining thermal conductivity various methods of experiment have been devised, of which Forbes' method of steady flow, and Angström's method of variable or periodic flow of heat, are the best known. The temperature gradient may be ascertained by means of thermometers placed in holes drilled at definite distances along the bar under experiment—a method only possible in bars of large cross-section; or by a thermo-electric couple sliding along the bar, as used by Wiedemann and Franz; or by a series of thermo-electric couples placed in small holes, or bound at definite distances, along the bar. The difficulty here is to be sure that the thermo-couple exactly attains the temperature of the particular section of the bar to which it is attached.

* An excellent résumé of the whole subject and of recent methods of experiment will be found in an article by Professor Callendar, F.R.S., in the Encyc. Brit., vol. xxvii., p. 188, *et seq.* See also Lee's paper on "Thermal Conductivity," Phil. Trans., 1892, p. 481, *et seq.*; and Preston's "Heat," chapter vii.

As the specimens of the alloys with which we had to deal were in the form of rods 102 cms. long and only half a centimetre in diameter, the gradient of temperature could only be found by thermo-couples, or by Ingenhouz's method of coating the rod with some easily fusible substance, such as wax, and measuring the length of wax melted along each rod when a permanent temperature was attained, one end of the rod being kept at a high and constant temperature. Both these methods were employed, and fairly concordant results obtained. The results at present obtained do not, however, profess to be more than a preliminary investigation; to obtain exact measurements, specimens of more suitable size and of more uniform purity are required, and also greater precautions would be necessary.

A rough experiment—made by fixing the rods vertically and placing their lower ends in a freezing mixture, which was renewed until a steady gradient of temperature along each rod was attained—showed, by the varying length of the deposit of hoar-frost on each rod, that wide differences existed in the thermal conductivity of these alloys.

A much better comparative series of experiments on the conductivity of heat through these alloys was made by carefully measuring the length of wax melted on each specimen, after one end of the rod had been kept for a long time at a high and steady temperature. When this permanent state is reached, then—if the rods, fixed horizontally, have the same cross-section and the same emissivity, and the same external conditions are preserved—the thermal conductivities of any pair of rods are proportional to the *squares* of the lengths of wax melted on each rod. The rods used were very nearly of the same cross-section, and their emissivities were rendered as nearly similar as possible by the fact that a thin coating of wax remained on each rod.

The method of experiment was as follows:—An iron vessel some 30 cms. long, 7 cms. deep, and 7 cms. wide, had eight holes, about 3 cms. apart, drilled along one side. Each hole was bushed with brass and asbestos, so as to fit closely the rods under experiment. One end of each of eight rods was fixed horizontally in these holes, and projected about 4 cms. inside the trough. The trough was filled with molten lead, and kept, as far as possible, at a uniform temperature by a suitable arrangement of Bunsen gas-burners, the temperature of the molten lead being taken by means of a special thermometer reading to 500° C. The rods were, of course, carefully screened from the heat radiated from the gas flames and trough, and from all surrounding air-currents. Before the experiment began, each rod was painted with a thin and uniform coating of paraffin wax. Molten lead was then poured into the trough, and its temperature kept fairly constant for a period of from one to two hours. The length of the wax melted on each rod was taken by

means of a pair of compasses from time to time, until it was found that a perfectly steady fall of temperature along each rod was attained.

In order to have a strictly comparative series of experiments, a standard rod of nearly pure iron, marked B by the maker, was included in each set of determinations, so that seven alloys, together with the standard B, were simultaneously tested. The rod B was of the same length and cross-section as the other rods, and was the specimen used as the standard in the previous determinations of the magnetic properties of these alloys. It contained only 0·03 per cent. of carbon, the total impurities amounting to less than 0·3 per cent. This rod, and all the iron alloys tested, had been carefully annealed. It would be desirable to repeat the experiments with hardened rods for the reason subsequently stated, and this we hope to do later on.

In the tables that follow, the squares of the lengths of the melted wax, that is, the relative thermal conductivities, are compared with the electrical conductivities of the same specimens, the iron standard used in both cases being taken as 100. It will be seen at once that both the thermal and electrical conductivities follow the same order.

This relationship between thermal and electrical conductivity, first remarked by Forbes, and more fully investigated by Wiedemann and Franz, has been the subject of much discussion and experiment. Wiedemann and Franz concluded that not only was the same order followed, but that the ratio of thermal conductivity k to electrical conductivity k' was the same for all metals and all temperatures. This, however, has been disputed by Tait, Weber, and Berget, though all agree that the order of conductivity in both cases holds good. Lorentz,* whose experimental results have been regarded as among the best yet obtained, found that this ratio k/k' was nearly constant for all good conductors at any one temperature, but increased in value nearly in the same proportion as the rise of absolute temperature. For 0° C., Lorentz found this ratio, when the conductivities were reduced to C. G. S. units, to be 1500 for the best conductors, and 2000 for the worst conductors. But, as Professor Callendar has pointed out, this relation cannot hold generally true, for in a specimen of cast iron—the thermal and electrical conductivities of which he himself carefully measured—this ratio amounted to 12,500.

All, therefore, that we can assert at present is that the ionic theory of conductivity receives considerable support from the fact that some close relationship does appear to exist between thermal and electric conductivity, in that the causes which affect the one appear also to affect the other; albeit the diffusivity of heat through a metal is enormously slower than the diffusivity of electricity.

* Wied. Ann. xiii., p. 422.

Beginning with a series of carbon steels, the results are given in Table I. The first column gives the mark attached to each specimen; the next the principal constituents (the complete analysis of each specimen will be found in Part I. of these researches *); the lengths in centimetres of wax melted when a steady temperature is attained are given in the third column, then the square of these lengths; and in the last two columns the relative thermal and electrical conductivities of the specimens are given, referred to the standard iron as 100. The last

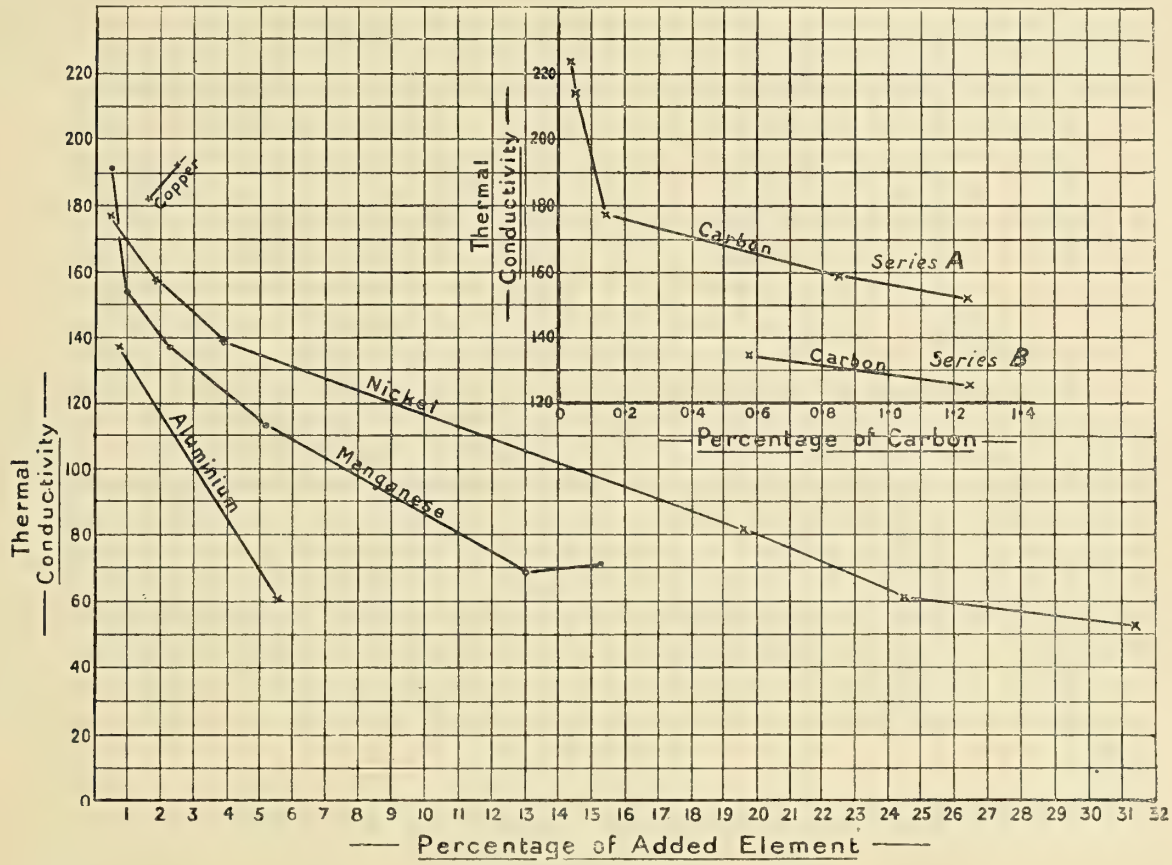


Fig. 1.

two specimens in Table I., Nos. 611 and 614, belong to a different series of carbon steels (called series B in our previous memoirs), containing higher manganese, and their conductivity is accordingly lower. In the right-hand top corner of fig. 1 these results are shown plotted in a curve, the abscissæ being percentages of carbon, and the ordinates L^2, the relative thermal conductivities.

* Sci. Trans. Roy. Dub. Soc., vol. vii., part iv., January, 1900.

TABLE I.

CARBON SERIES.

Mark.	Principal Constituents.	L.	L^2.	Relative Conductivity.	
				Thermal.	Electrical.
B	99·71 Fe; 0·03 C, . . .	15·0	225	100·0	100·0
L S S	99·72 Fe; 0·05 C, . . .	14·6	213	94·6	96·3
1166 A	99·78 Fe; 0·14 C, . . .	13·3	177	78·6	85·5
1392 A	98·66 Fe; 0·85 C, . . .	12·6	159	70·6	66·5
1392 G	98·51 Fe; 1·23 C, . . .	12·3	151	67·1	62·0
611	98·35 Fe; 0·58 C, . . .	11·6	134	59·6	52·6
614	97·67 Fe; 1·25 C, . . .	11·2	126	56·0	46·3

As was stated in a previous paper, it is possible (other conditions being the same) to estimate the amount of carbon present in carbon steels by the electric conductivity of the specimen. Our results have been quoted in a recent paper by M. Benedicks, who has confirmed the foregoing opinion.* It would be desirable to repeat the experiments on electric and thermal conductivity, especially of the carbon steels, with specimens quenched at a *definite*, high temperature, for only thus can the true characteristics of each alloy be exhibited. A series of extremely pure carbon steels is now being prepared at the Hecla Steel Works; and we hope to repeat our experiments with these in the quenched as well as the annealed states.

In Table II. are given the results with some of the manganese steels: it will be borne in mind that the series of specimens in each Table were tested simultaneously, so that they are strictly comparable.

* I am glad to draw attention to Dr. Carl Benedicks' valuable and laborious investigations, entitled *Recherches physiques et physico-chimiques sur l'acier au carbone*, published this year in Upsala. One portion of this important work, the author states, is founded on the fact that "la résistance électrique permet une évaluation exacte de la teneur en carbone de trempe." It is by this means he arrives at the conclusion that the solubility of carbon in iron, when excess of carbon is present, is 0·27 per cent. at the ordinary temperature. Furthermore, Dr. Benedicks, by the collation of our experimental results with those of his own and of some others, has arrived at the important conclusion that "des quantités équivalentes des éléments étrangers, dissous dans le fer, causent le même accroissement de résistance," and hence gives a provisional formula by which the electric resistivity of any steel can be calculated when its chemical composition is known—a remarkable conclusion which awaits further verification.

Table II.

Manganese Series.

Mark.	Principal Constituents.	L.	L^2.	Relative Conductivity.	
				Thermal.	Electrical.
B	99·71 Fe, ...	15·0	225·0	100·0	100·0
48	99·30 Fe; 0·50 Mn, . .	13·8	190·4	84·6	75·5
4147	98·76 Fe; 1·00 Mn, . .	12·4	153·8	68·3	46·4
53	97·34 Fe; 2·25 Mn, . .	11·7	136·9	60·8	37·4
32	94·53 Fe; 5·15 Mn, . .	10·6	112·4	50·0	29·2
1323 C	94·46 Fe; 5·40 Mn, . .	10·7	114·5	50·8	32·4
1338	86·74 Fe; 13·00 Mn, . .	8·3	68·9	30·6	17·8
1379	84·64 Fe; 15·20 Mn, . .	8·4	70·6	31·3	16·8

These results are plotted in the curves shown in fig. 1, p. 113. The rapid drop in conductivity for the first small increments of manganese occurs also with carbon, and is true of both thermal and electric conductivity.

The next table gives the results with some of the nickel steels. Unfortunately the specimens, though purer than ordinary nickel steels, contained a considerable amount of manganese, which impurity, as just remarked, seriously impairs the conductivity. A given percentage of nickel alloyed with iron has far less effect in reducing both the thermal and electrical conductivity of iron than the same percentage of manganese or carbon. These results are shown plotted in fig. 1.

Table III.

Nickel Series.

Mark.	Principal Constituents.	L.	L^2.	Relative Conductivity.	
				Thermal.	Electrical.
B	99·71 Fe, ...	15·0	225	100·0	100·0
1397 B	98·65 Fe; 0·58 Ni, . . .	13·3	177	78·8	61·0
1287 D	97·00 Fe; 1·92 Ni; 0·72 Mn,	12·5	157	69·8	53·2
1287 E	95·14 Fe; 3·82 Ni; 0·65 Mn,	11·8	139	61·8	45·6
1287 I	87·28 Fe; 11·39 Ni; 0·93 Mn,	9·0	81	36·0	30·5
1287 K	78·97 Fe; 19·64 Ni; 0·93 Mn,	9·0	81	36·0	27·9
1287 L	74·03 Fe; 24·51 Ni; 1·00 Mn,	7·8	61	27·2	24·1
1449 A	67·08 Fe; 31·40 Ni; 0·82 Mn,	7·2	52	23·1	12·7

In Table IV. are given the results with some tungsten, aluminium, and silicon steels. Tungsten was found to have the least effect of any element, so far tried, in reducing the electrical conductivity of iron, whilst aluminium had the greatest effect. The same thing comes out here: the curve for the aluminium-iron alloys is shown in fig. 1. The last specimen in Table IV., 1414 B, was an alloy which was found to have the highest electric resistivity in the whole series of upwards of 100 iron alloys; it is also found to have the highest thermal resistivity of any of the alloys tested. This alloy, which is a nickel manganese steel, owing to its very low heat-conducting power, might be found invaluable for the inner lining of the cylinders of steam-engines; such a lining would, as is well known, greatly increase the efficiency of the steam-engine.

TABLE IV.

Mark.	Principal Constituents.	L.	L^2.	Relative Conductivity.	
				Thermal.	Electrical.
B	99·71 Fe, ...	15·0	225	100·0	100·0
1392 H	99·02 Fe; 0·78 C, . . .	13·5	182	81·0	75·0
1294 F	98·73 Fe; 1·00 W, . . .	13·1	172	76·4	72·3
1294 H	95·94 Fe; 3·50 W, . . .	12·2	149	66·3	61·0
1167 D	98·98 Fe; 0·75 Al, . . .	11·7	137	60·9	49·5
1167 I	94·08 Fe; 5·50 Al, . . .	7·7	61	27·2	15·9
898 H	94·24 Fe; 5·50 Si, . . .	8·5	72	32·0	16·7
1414 B	69·36 Fe; 25·0 Ni; 5·04 Mn, .	7·1	50	22·2	12·2

In Table V. is given a miscellaneous collection of steel alloys, all containing copper. It will be noticed that none of them has a higher thermal or electrical conductivity than the iron standard. But unfortunately these alloys all contain either high carbon or manganese impurity; to ascertain the true effect on the conductivity of iron produced by alloying it with copper, pure alloys must be made, or a comparison instituted with the conductivity of a carbon steel having the same percentage of carbon or manganese. Under these circumstances it will probably be found that copper has very little effect on the conductivity; it may slightly raise it.

TABLE V.

Mark.	Principal Constituents.						L.	L^2.	Relative Conductivity.	
	Fe.	C.	Ni.	Mn.	Cr.	Cu.			Thermal.	Electrical.
B	99·71	—	—	—	—	—	15·0	225·0	100·0	100·0
1264 A	99·37	0·68	—	0·36	—	1·59	13·5	182·0	81·0	72·8
1264 B	96·59	0·59	—	0·32	—	2·50	13·9	192·0	85·5	75·5
1263 C	95·92	0·17	—	1·04	—	2·87	12·8	164·0	72·9	62·1
1255 A	91·60	0·85	—	—	5·75	1·80	10·5	112·0	49·8	34·8
1252 B	91·32	0·18	5·75	—	—	2·75	9·4	88·5	39·4	28·6
1240	96·25	0·25	—	2·00	—	1·50	11·1	123·0	54·7	43·0
1260 A	88·61	0·64	—	8·00	—	2·75	7·9	62·5	27·8	21·0

As already stated, all the foregoing results can only be regarded as preliminary, the method of experiment not allowing more than approximate accuracy. We next adopted the method of obtaining the gradient of temperature by means of thermo-couples lashed on to the rods, one end of the rod being kept at a uniformly high temperature. This part of the research was entrusted to Mr. S. A. Edmonds, a senior student in the physical laboratory of the Royal College of Science for Ireland, who, on completing his College course, obtained a research Bursary. The results, whilst in general agreement with those already given, are of a higher degree of accuracy, owing to the method employed.

To render the emissivity as far as possible uniform, the surface of each rod was cleaned in weak acid, then washed and brightened by friction with fine emery. One end of the rod was heated by means of an asbestos-jacketed steam cylinder, the rod passing through a screwed stuffing-box, and supported in a horizontal position. The portion of the rod outside the steam cylinder was enclosed in a trough made of a double thickness of cardboard separated by cotton wool, a cover being placed over the trough, so that the cooling effect of air currents was as far as possible avoided. The thermo-couples employed to take the temperature along the rod were made of German-silver and iron wires of No. 28 B.W.G.; the tightly twisted ends of each couple were lashed round the rods, and the free ends dipped into a pair of mercury cups immersed in a large mass of

water; from these mercury cups wires led to the galvanometer. A moving coil dead-beat reflecting galvanometer was employed with an extremely good concave mirror, whereby scale-readings of a fraction of a millimetre could be made. As the coil of the galvanometer had a high resistance, any error due to variation in the resistance of the leads was eliminated. The following is an extract from Mr. Edmonds' report:—

"After various trials, an effective way was found of securing good contact between the thermo-couples and the rod; six couples were employed, fixed at definite and exactly similar distances along the rod under experiment. Each thermo-couple was beforehand carefully calibrated by a standard mercurial thermometer; within the limits of temperature used the calibration curve was a perfectly straight line; from this curve the temperature difference corresponding to any given deflection was read off. From the temperatures given by the couples fixed along the rod a curve was drawn for each rod, having excess of temperature, above the air as ordinates and distances from the source of heat as abscissæ. From these curves the relative conductivities were obtained. A nearly pure copper rod, having an electrical conductivity 96 per cent. of Matthiessen's standard pure copper, and corresponding in length and diameter to the specimens of iron alloys used, was employed as a standard of reference. The absolute thermal conductivity of this copper standard was subsequently determined, both by the method of Forbes and by Angström's method of the periodic flow of heat. The latter method involved much troublesome reduction of the observations; the value found for the absolute conductivity of this specimen of pure copper in C. G. S. units was 1·041.

"Assuming the temperatures along any two rods to be given by $\theta = A\epsilon^{\mu x} + B\epsilon^{-\mu x}$ and $\theta = A\epsilon^{\mu' x} + B\epsilon^{-\mu' x}$, and the temperatures at points x, $x+l$, $x+2l$, along one rod θ_1, θ_2, θ_3, and at the same points along the second rod θ_1', θ_2', θ_3', then it can be shown that

$$\frac{\mu}{\mu'} = \frac{\log_\epsilon(n + \sqrt{n^2 - 1})}{\log_\epsilon(n' + \sqrt{n'^2 - 1})},$$

where $n = \dfrac{\theta_1 + \theta_3}{2.\theta_2}$ and $n' = \dfrac{\theta'_1 + \theta'_3}{2\,\theta'_2}$.

"From the equation for the flow of heat along the rod we have

$$\mu^2 = \frac{2E}{Kr} \quad \text{and} \quad \mu'^2 = \frac{2E'}{K'r'},$$

"where E is the surface emissivity of the first rod, r = its radius, K = its

absolute conductivity, and E', r', K' the same for the second rod. Putting in these values we have

$$\sqrt{\frac{K'Er'}{KE'r}} = \frac{\log(n + \sqrt{n^2 - 1})}{\log(n' + \sqrt{n'^2 - 1})}.$$

"The surfaces of the two bars had the same treatment, so that E may be assumed to be equal to E'; and we get

$$\sqrt{K'} = \frac{\log(n + \sqrt{n^2 - 1})}{\log(n' + \sqrt{n'^2 - 1})} \sqrt{K} \frac{\sqrt{r}}{\sqrt{r'}}.$$

"Taking for our standard copper rod the values $K = 100$ and $r = 0{\cdot}25$ cms., this becomes

$$\sqrt{K'} = \frac{\log(n + \sqrt{n^2 - 1})}{\log(n' + \sqrt{n'^2 - 1})} \times \frac{5}{\sqrt{r'}}.$$

"From this formula the conductivities of the rods were found relative to the copper standard, where n for each rod was obtained from the temperatures at points distant 5 cms., 7·5 cms., and 10 cms. from the source of heat, these being taken from the temperature gradient curve previously drawn for each rod."

The first experiment made was with the specimen of very pure Swedish charcoal iron (S.C.I.), referred to in our previous memoirs. The total impurities in this iron amounted to only 0·15 per cent. Its thermal conductivity was found to be 23, calling the standard copper 100.

It has already been stated that the specimens had all been carefully annealed; the difference in conductivity caused by annealing is very marked, especially in the nickel steels. This was shown in our previous papers, where in the case of one composite iron containing 14 per cent. of nickel, 5 per cent. of manganese, and 2 per cent. of aluminium, alloyed with 78·5 per cent. of iron, the annealed specimen had an electric *resistivity* of 48 microhms per c.c., whilst the same specimen when hardened had the enormous resistivity of 89 microhms, an increase of 85 per cent. produced by hardening. In like manner, thermal conductivity is increased by annealing. The nickel steel 1287 K, containing 19·6 per cent. of nickel, was found to have a relative thermal conductivity of 11·1 in the annealed state, and of 7·1 in the hardened state, copper being 100: that is, an increase of over 54 per cent. in conductivity produced by annealing.

The results obtained by Mr. Edmonds with 20 of the iron alloys are shown grouped together in the following table, the thermal and electrical conductivities of the *same rod* being each compared with the same *copper rod* taken as 100.

TABLE VI.

Series.	Maker's Mark.	Percentage Composition.				Relative Conductivity. Copper = 100.	
		Fe.	C.	Mn.	Ni.	Thermal.	Electrical.
Pure Commercial Iron, . .	S C I	99·85	0·03	trace	—	23·01	16·80
Manganese Series, . . .	48	99·30	0·20	0·5	—	21·32	12·14
,, ,, . . .	4147	98·76	0·24	1·0	—	16·03	7·45
,, ,, . . .	39	95·64	0·36	4·0	—	14·74	6·12
,, ,, . . .	1323 C	94·46	0·15	5·4	—	14·38	5·21
,, ,, . . .	1338	86·74	0·26	13·0	—	8·77	2·86
,, ,, . . .	1379	84·64	0·15	15·2	—	8·63	2·70
Nickel Series, . . .	1397 B	98·65	0·26	0·18	0·58	18·54	9·80
,, ,, ...	1287 D	97·01	0·14	0·72	1·92	18·09	8·56
,, ,, ...	1287 E	95·14	0·19	0·65	3·82	16·00	7·35
,, ,, ...	1287 K	78·97	0·19	0·93	19·64	11·14	4·49
,, ,, ...	1449 A	67·08	0·70	0·82	31·40	6·74	2·04
Nickel-Manganese Steels, .	1109 D	80·16	0·89	5·04	14·55	5·86	2·09
,, ,, . .	1414 B	69·36	0·60	5·04	25·00	4·39	1·97
Tungsten Series, . . .	1294 F	98·73	0·16	0·11	W. 1·00	19·66	11·63
,, ,, . . .	1294 H	95·94	0·28	0·28	3·50	19·18	9·80
,, ,, . . .	1294 L	83·46	0·76	0·28	15·50	14·87	6·54
Tungsten-Manganese Steels .	687	94·10	0·40	2·25	3·25	15·08	6·32
,, ,, .	683	85·23	1 50	3·25	10·00	14·55	5·71
Chromium Steels, . . .	993	97·10	0·90	—	Cr. 2·00	15·31	7·25
,, ,, . . .	1177 N	89·45	1·09	—	9·50	11·69	4·59

Omitting the composite alloys, these results are plotted in fig. 2, where the abscissæ represent the percentage increase of the element added to the iron, and the ordinates the respective thermal and electrical conductivities relative to copper as 100. These curves show by inspection that the effect of alloying iron with another element, even a better conductor, reduces both the thermal and electrical conductivity. Compared with copper, the electrical conductivity of pure iron and its alloys is throughout lower than their thermal conductivity. It will, however, be noticed that the general trend of the curves for any one series of alloys is the same thermally and electrically. That is to say, not only is the

order of conductivity the same in both series, but equal increments of any given element appear to produce a corresponding diminution of conductivity for both heat and electricity. The small irregularities in the heat curves are not more than might be expected in these preliminary experiments, and where they occur in both sets of curves are due to the varying impurities of the alloy. As was noticed in our experiments on electrical conductivity of these iron alloys, the first

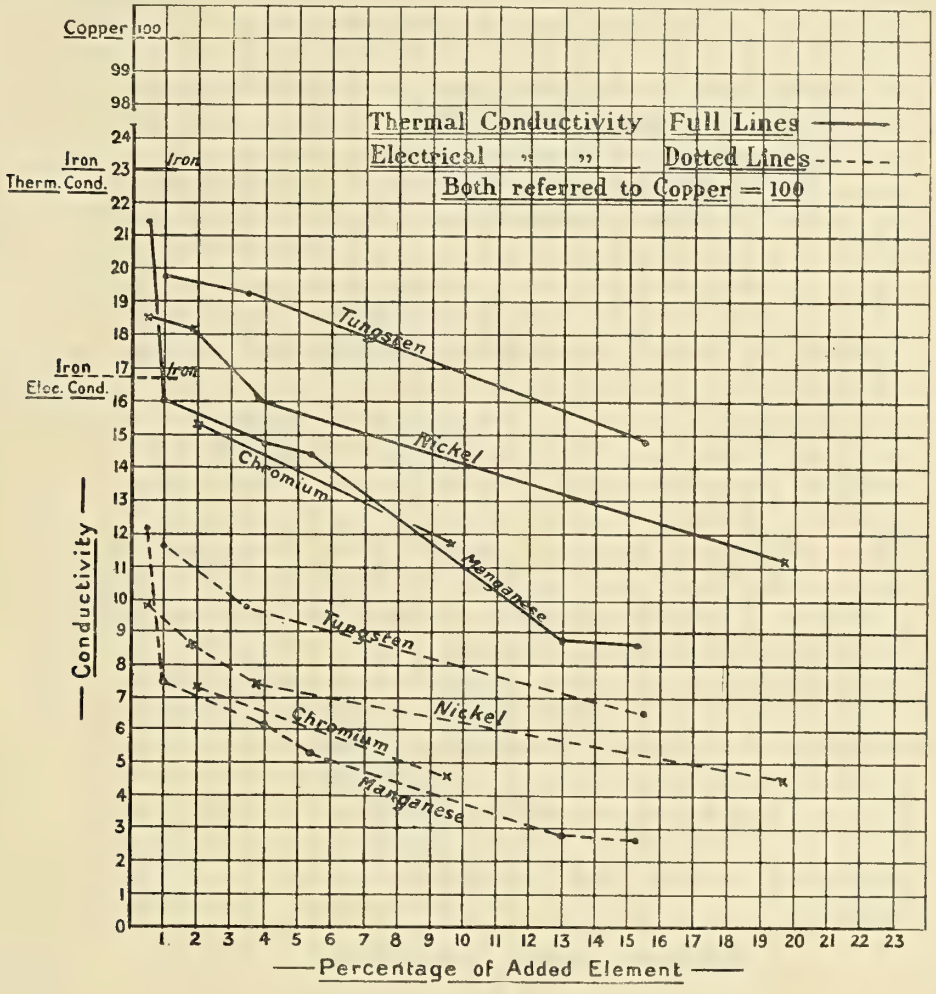


Fig. 2.

addition of any given element, especially of carbon or manganese, has the greatest effect in reducing the conductivity.

In a paper by one of us, read before the Royal Society,* it was pointed out that the diminution in conductivity of iron, by the addition of a given percentage of an element which can be alloyed with it, is closely related to the atomic mass of the added element. The greater the atomic mass of the added element, the less

* Proc. Royal Society, 1902, vol. lxix., p. 480.

appears to be the reduction in conductivity produced in iron alloyed with that element. Thus the tungsten-iron alloys are better conductors than the aluminium-iron alloys; the latter, in fact, having a very high resistivity, the atomic mass of aluminium being very low. This relation appears to hold true of the thermal as well as of the electrical conductivity of these alloys.

The ratio k/k' of the electrical to the thermal conductivity is best exhibited in the curve shown in fig. 3, where the ordinates represent the thermal and the abscissæ the electrical conductivities given in Table VI., and referred to copper as 100. The curve, it will be seen, is a fairly smooth one, and of parabolic form, except when the conductivities are very low. In that case the curve becomes

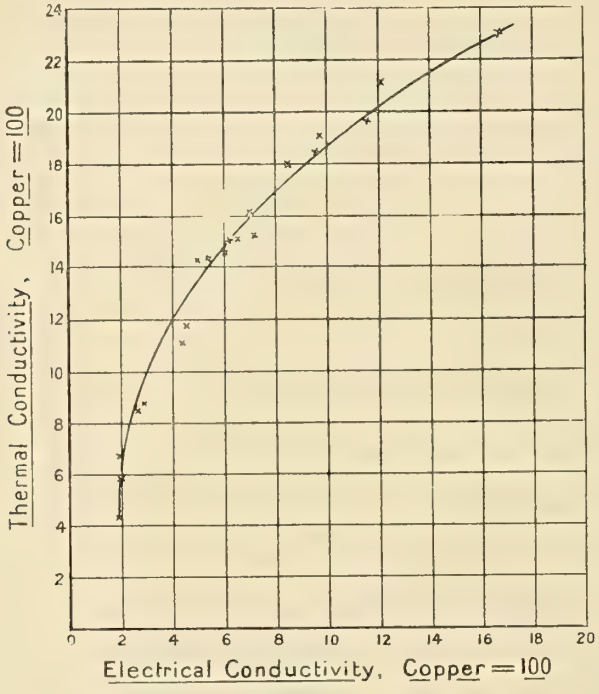


FIG. 3.

much steeper, that is, the magnitude of the ratio increases, which confirms Lorentz's statement. The numerical value of the ratio, when the conductivities are expressed in C. G. S. units, is, however, much greater than that given by Lorentz. More exact measurements of the thermal conductivities in unannealed specimens of these alloys are urgently needed. Unfortunately we have been unable so far to carry this out, as Mr. Hadfield wanted the specimens at Sheffield. Moreover, owing to the present overcrowded state of the physical laboratory at the College of Science, it has been found impossible to carry on satisfactorily any original physical work for some time past, as the whole of the available space is occupied by students.

PART V.

Micro-Structure.

In the previous Parts of these researches we have shown that a small percentage of the non-magnetic metals silicon and aluminium, alloyed with iron, raised the magnetic permeability of the alloy beyond that of the purest commercial iron; and on the other hand, a metal like manganese, itself feebly magnetic, rendered iron practically non-magnetic when alloyed in sufficient quantity with iron.* In Part III. we remarked:—

"... The question remains to be considered, what is the chemical or physical change produced in iron by the presence of other metals; so that on the one hand the magnetic permeability of the alloy is practically destroyed by the added element, and on the other increased. From their strong chemical affinities for oxygen and the halogens, silicon and aluminium may act both chemically and physically. By combining with oxygen or with dissociated gas they may render the iron freer from oxide of iron, or give it a closer texture, and prevent fine honeycombs being formed. Microscopic examination of sections of all these alloys is needed; such examination cannot fail to throw light on the relation between magnetic qualities and the size and nature of the grain, or the crystalline structure of the alloys."†

This microscopic examination was most kindly undertaken by Dr. Glazebrook at the National Physical Laboratory. Sections of the alloys were made and polished, and micro-photographs taken before and after etching. The work, which was very laborious, Dr. Glazebrook entrusted to Dr. Carpenter, to whom our best thanks are due. Another interesting and anomalous point required clearing up. When the very high permeability rod, 1167 H, containing $2\frac{1}{4}$ per cent. of aluminium alloyed with iron, was carefully turned down to a smaller diameter, then subsequently re-annealed, and new B and H curves taken, the magnetic permeability was no longer so high, scarcely exceeding that of the best iron. The turned-down rod was now drawn down to a still smaller diameter, carefully annealed, and again tested magnetically; its permeability was found to be even below that of the purest Swedish charcoal-iron, S.C.I., which had been in like manner reduced in diameter and re-annealed. Furthermore, fresh specimens of the aluminium-iron alloy, prepared from new castings, were not as good, magnetically, as the first specimen. These anomalies, it was hoped, microscopic

* Sci. Trans. Roy. Dub. Soc., vol. vii., January, 1900, p. 122; vol. viii., September, 1902, p. 6.

† *Ibid.*, vol. viii., September, 1902, p. 21.

examination might clear up. From the accompanying report, by Dr. Carpenter, it will be seen that our expectation has, to a large extent, been realized. An outer layer of nearly pure iron, ferrite, was found in the original rod of the aluminium-iron alloy; but this layer had been removed by the act of turning down the specimens.

The accompanying report from the National Physical Laboratory refers to the following specimens, the chemical analyses of which were made in the laboratory of the Hecla Steel Works, Sheffield.

S.C.I. (Swedish charcoal-iron), containing 99·89 Fe, 0·03 C, 0·07 Si.

1167 H. (Aluminium-iron alloy), 97·33 Fe, 0·24 C, 0·18 Si, 2·25 Al.

898 E (Silicon-iron alloy), 97·3 Fe, 0·2 C, 2·5 Si.

NOTES EXPLANATORY OF THE PHOTO-MICROGRAPHS.*

PLATE IX.

Rod S.C.I.

"Photo-micrographs 1 and 2 represent the structures in different parts of the same transverse section. Deeply etched in 1 % nitric acid in absolute alcohol.

"Dark area shown in No. 1 about $\frac{3}{8}$ of the total area. [It will be noticed that the white patches, ferrite, are fairly uniformly distributed. In the high magnification of No. 2, hexagonal or polyhedric grains of ferrite are seen.]

Rod 1167 *H.*

"Photo-micrograph No. 3 (lightly etched) shows the structure as one proceeds from the edge (white) to the centre. Ferrite, granular pearlite, and nodules are all present; and although the structure is different from that of the ingot, it is only such as might be expected from the thermal and mechanical treatment to which the rod has been subjected. Attention is drawn here to the fact that a layer of ferrite, varying in diameter from about $\frac{1}{6}$ to $\frac{1}{24}$ mm., occurs on the outside of the rod right round. No. 4 (magnification = 2520 diameters) shows small nodules forming a ring pattern on a ground-mass of what in No. 3 is seen to be granular pearlite. It seems probable that the nodules are the oxide of aluminium.

* The circles of the micro-photographs represent, of course, only a minute portion of the cross-section of the rod. When the micro-photograph is taken at the edge, this word is added.

Plate X.

Rod 1167 *H. Nos.* 1 *and* 2, *the same as Nos.* 3 *and* 4, *Plate IX., only with deep etching.*

"This reveals the presence of grains of very varying size. Some are about $\frac{2}{3}$ mm. in diameter; others are less than $\frac{1}{50}$ mm. in diameter. The white areas in relief seen in No. 2 are probably aluminium which has been rendered passive by the treatment with nitric acid. The tiny nodules of oxide of aluminium are best seen in the high-power photo-micrographs. The fact may be mentioned that bar 1167 H was almost unattacked by a 5 per cent. solution of picric acid in absolute alcohol—even after prolonged treatment.

Rod 1167 *H turned, and then drawn down. No.* 3, *deep etching. No.* 4, *light etching.*

"There is no layer of ferrite running round the edge. No. 3 was taken right on the edge. [The outside edge of the rod is seen in the lower flattened edge of the disc.]

Discussion of Results.

"It may be considered that the photo-micrographs throw some light on the results recorded in Barrett, Brown, and Hadfield's paper; but they cannot be said to do more than this. The experimental basis is not sufficiently broad. As an illustration of what is meant by this, it is only necessary to point out that, according to analyses recorded in the paper just referred to, the carbon content of rod 1167 H is eight times, and that of rod 898 E is seven times, the carbon content of rod S C I. The different values for magnetic permeability found for these three rods may be connected not only with the presence or absence of aluminium or silicon, but also with that of carbon, and the mutual influences of these on one another as well as their particular influence on the iron. Accordingly it seems most desirable to extend the number of alloys already experimented with, in such a way that, while, *e.g.*, the aluminium content is kept constant, the carbon content is varied, and *vice versa*. In this way the influence of each constituent can be found.

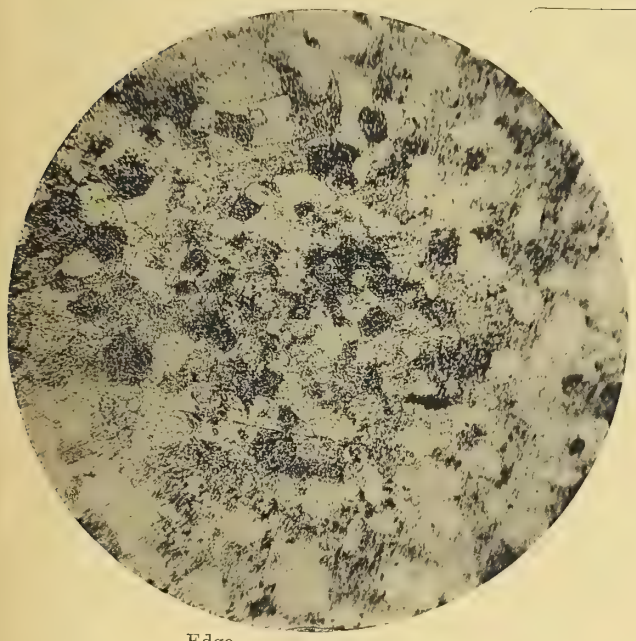
"But considering the results of the photo-micrographic examination broadly, two striking facts are brought to light, which differentiate rod 1167 H from rods 1167 H drawn down, 898 E, and S C I, and which may be connected with the high magnetic permeability of the first-named rod. (1) A layer of ferrite, *i.e.* pure iron of somewhat varying thickness, probably about one-tenth of a millimetre on the average, occurs on the outside of the rod the whole way round. It forms

about 10 per cent. of the total area. (2) White patches in relief are seen in certain photo-micrographs of 1167 H. These are probably aluminium which has either not dissolved in the iron, or, if it has dissolved in the molten alloy, has separated out on cooling. If this is the case, the iron is purer than that in 898 E, which appears to retain the whole of the silicon—other than that oxidised to silica—in solution. Part of the aluminium has been oxidised to alumina. It is curious that the white patches in relief, distributed fairly uniformly through 1167 H, are absent from 1167 H drawn down.

"The light etching effects obtained with rods 1167 H turned down and 898 E are very noticeably similar. They indicate that minute particles of alumina and silica respectively are distributed through the rods. The action of aluminium and silicon in these rods may consist (as suggested in Barrett, Brown, and Hadfield's paper) in removing oxygen or decomposing oxide of iron, and thus purifying the iron, thereby raising its magnetic permeability. If this is really the case, it must be taken into account in considering the cause of the high magnetic permeability of 1167 H."

Swedish Charcoal Iron.
Deeply etched.

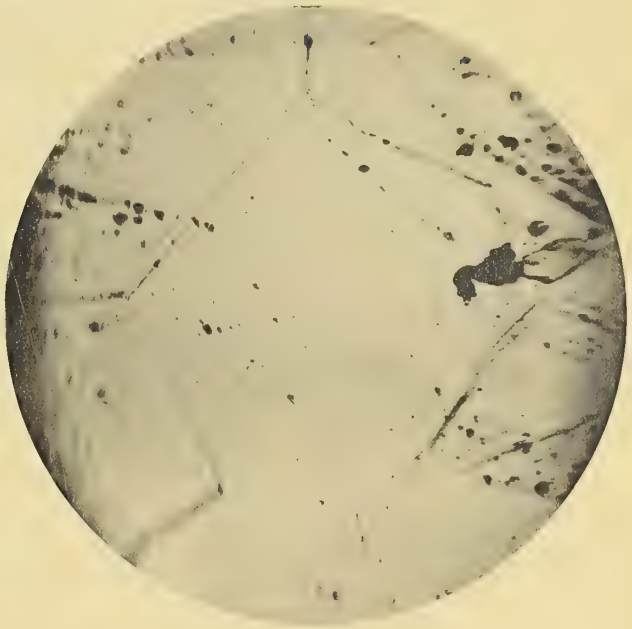
× 120.



Edge.

1

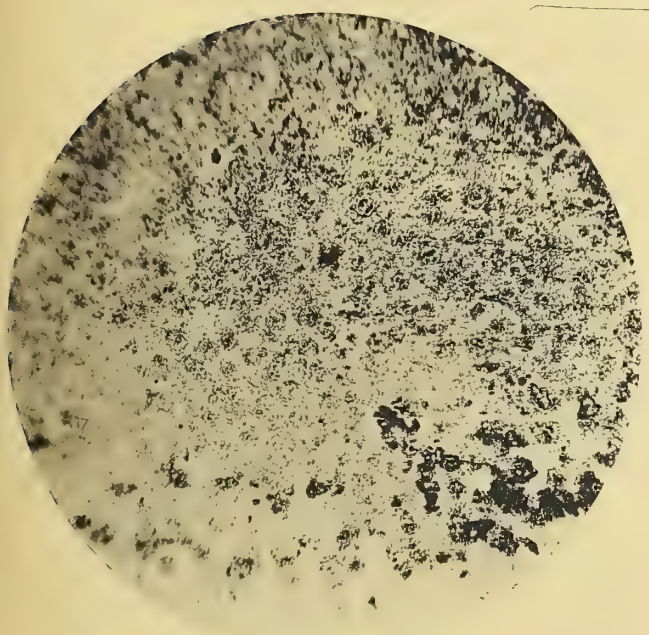
× 1000.



2

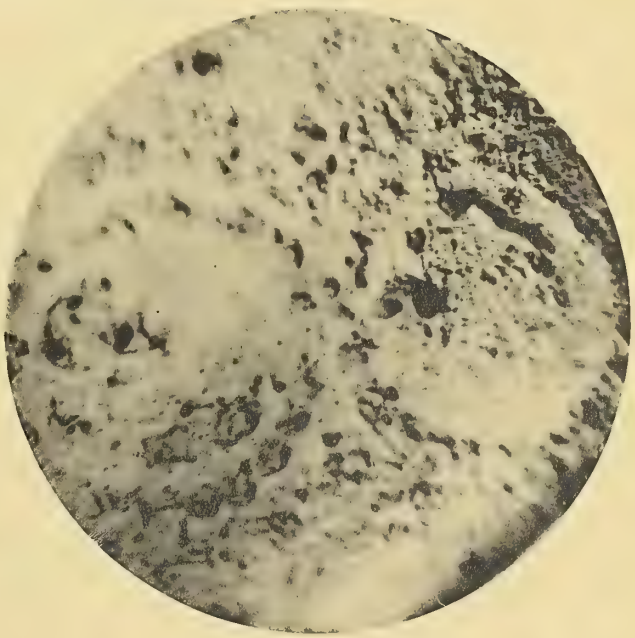
Aluminium Iron.
Lightly etched.

× 120



Edge.
Ferrite layer. 3

× 2520



4

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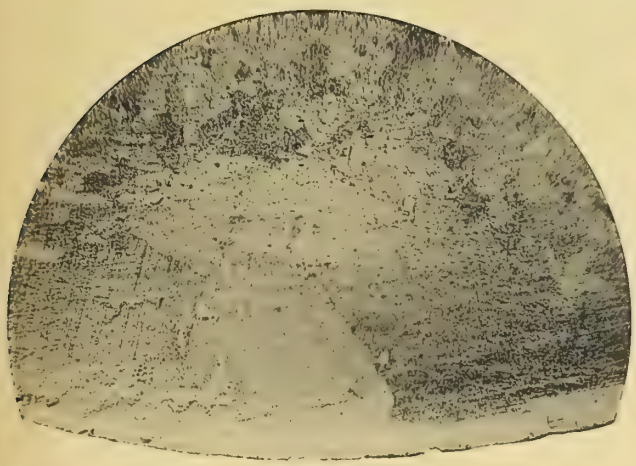
× 120.



Edge. Ferrite layer.

1

Aluminium Iron.
Deeply etched.

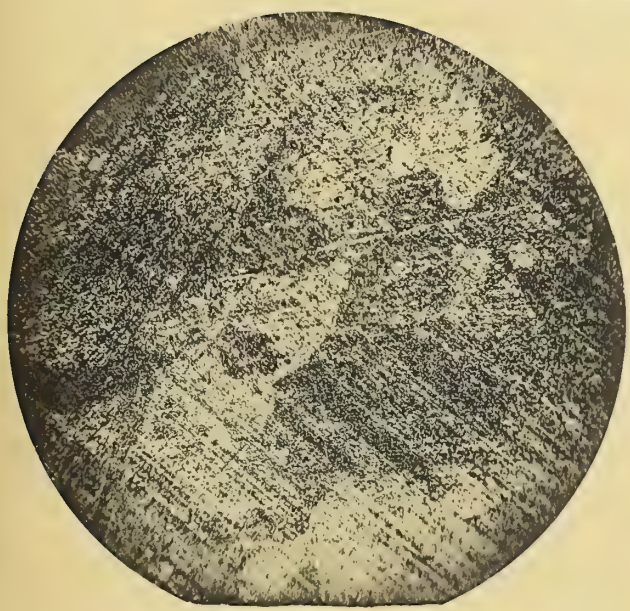
× 120.

2

× 120.

The same specimen
turned and drawn down.

× 124.



Edge.
No Ferrite layer. 3

4

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[SEPTEMBER, 1904.]

THE

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X.

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BY

F. E. HACKETT, B.Sc.

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THE PHOTOMETRY OF N-RAYS.

By F. E. HACKETT, B.Sc.

[COMMUNICATED BY PROFESSOR J. A. M'CLELLAND, M.A., F.R.U.I.]

[Read, May 17, 1904.]

I.

It is rare in physical science to find a divergence of opinion on a matter which can be subjected to experiment. Perhaps never before was such a difference of opinion more justifiable than in the case of N-rays. The repeated failures to observe any of their effects seemed to totally disprove their objective existence. Since the observation of a change in dimly-illuminated objects is at present the only means of detecting the N-rays, it is important to know why the effect described by Blondlot is invisible to so many observers.

This paper is an attempt to give the explanation, and to point out how the causes which affect such observations may be minimised.

The first part of the paper gives some measurements of the subjective effects of the dark-adapted eye. This, it is hoped, will dispel some of the vagueness which hangs about these effects. The second part deals more particularly with the N-rays themselves. As most observers seem to have used screens of calcium sulphide made phosphorescent by daylight, it has been thought useful to give some measurements of the effect under these conditions. These show that the effects are so small that they cannot be observed without special training. The conclusion of the paper describes the manner in which any person can see the effect without trouble, and test the objective existence of N-rays. The method of comparison is used, and the screens must be very dim. Some estimations of the effect under these conditions are given. No measurements are given except those which can be done with a certain amount of precision. The experiments have been carefully repeated after intervals ranging from one to two months, with concordant results.

Before proceeding to the actual experiments, it is desirable to discuss the special point of view adopted in this paper regarding the observation of brightness with the dark-adapted eye. The main use of a phosphorescent screen is simply to afford us very conveniently a dimly-lighted surface. It is necessary in the

N-ray effect to look for a small change in such a surface. We have then to remember brightness—an exceedingly difficult thing to do. If this were our sole guide in these observations, Blondlot's experiments would deserve the severest criticism.

But the eye, consciously or unconsciously, guides itself by another property of such a surface. Looking at a dimly-lighted surface, we find it difficult to distinguish its outline unless we are near. The further off we go, the more blurred and indistinct does it become; an increase of brightness diminishes the indistinctness until the whole outline comes out clearly again. By paying special attention to this aspect of the matter, I hope to be able to show why so many have found such observations difficult, and how, by taking advantage of this property, it is possible with practice to make such observations easy.

The object observed in many experiments was simply a diaphragm, with two vertical slits fixed in front of a phosphorescent screen. The slits were separated by a distance equal to their own width, which varied from 2 to 3 mm. At a short distance, the two slits came out very distinctly, separated by a black line. If now the screen is gradually moved away from the eye, the black interval between the slits gradually gets more and more indistinct, and finally disappears. The two slits appear to be a uniform spot of light. In this condition, or rather slightly before this condition is reached, the screen is extremely sensitive to changes of brightness. A slight increase of brightness causes the re-appearance of the line between the slits.

It is difficult to say what are the conditions of intensity and distance when this indistinctness takes place. If we look upon the outline as a series of points, the illumination of the outline will then follow the same law as the illumination of a point—that is, the illumination or indistinctness of the outline will be dependent on the intrinsic brightness, and the inverse square of the distance. Indicating the intrinsic brightness by I, and the distance of the screen from the observer by R, we might say, then, the indistinctness would be some function of $\frac{I}{R^2}$. So that under the same conditions of indistinctness $\frac{I}{R^2}$ would be constant. Such a law would be in accordance with the fact that the greater the brightness of the screen, the greater its distance from the eye in order that it should be equally indistinct.

A simple experiment was made to see if there was any foundation for assuming such a law. Two slits, 2 mm. apart, were cut in a sheet of cardboard, and covered with white paper. This was illuminated from behind by a very minute gas flame. The arrangement gives two bright slits separated by a dark line,

which appeared to an observer exactly the same as the diaphragm and screen described above, and varied in the same way. The distance at which the middle black line vanished for a given distance of the gas flame from the screen was determined. If this law be true, then the product of these two distances should be constant. It was found that this condition was roughly satisfied, so that it might be used in getting some general idea of changes in brightness in the manner adopted in this paper.

Many measurements were made on this assumption. Such measurements are necessarily very rough. They are not intended to be quantitative, but only to give some idea of the change of brightness observed. It is a great obstacle in experiments in N-rays that one does not know what change to expect. It is hoped some of the numbers given may be of use in forming an estimate of the changes in brightness which take place.

The considerable variation of sensitiveness over the retina when the eyes are being used in darkness is well known. Lummer, in an address to the German Physical Society,* pointed out the great necessity of paying attention to this property. It was found necessary, at a very early stage in this investigation, to get more definite information on this and other peculiarities of the dark-adapted eye, than the vague statements which are given, even by the best authorities on the physiology of the eye. This property of the retina was investigated by drawing lines of equi-sensitiveness. Captain (now Sir William) Abney has given such curves in his paper entitled "The Sensitiveness of the Retina to Light and Colour,"† but these, however, were only got for the external parts of the retina in daylight. He suggested the name "isolumes" for these curves.

The isolumes for the dark-adapted retina cannot be expected to have any relation to those got in daylight. It was found quite simple to take observations around and within the yellow spot. They were made in a manner which is the same in principle as Abney's method, and require some trouble and patience.

In applying this method, the object used was the combination of diaphragm and screen already described. The slits are, however, of such a size that when they appear blurred their image on the retina is not larger than the fovea centralis, which is a circular area 3 mm. in diameter. A bright spot of light (some calcium sulphide at the end of a rod) is used to fix the position of the eye. When the eye is looking at the bright spot, its image falls on the fovea. In the experiment the eye was made to follow the spot of light as it was moved horizontally across the diaphragm away from the slit. As the distance between the spot of light and the slits increased, the latter became more and more distinct, until the black line

* *Nature*, Feb. 18th, 1904.

† Phil. Trans., vol. cxc., 1897, A, p. 155.

could be clearly distinguished. The position of the bright spot of light was then marked. The process was repeated on the other side of the slits. The spot of light was moved, until apparently the slits came out just as clearly. The same thing was done above and below the slits. These positions could be clearly determined, as the change of brightness, though slow at first, increases rapidly afterwards. From these readings we could take a plan of the retina, and plot a curve in it which would be a line of equi-sensitiveness.

Another observation yet remains to be mentioned. After the four positions had been determined, the screen was moved forward, until apparently the slits came out as clearly as before. Assuming the law of inverse squares, the apparent increase of brightness for small changes is equal to twice the distance moved forward, divided by the total distance; in symbols $\frac{2D}{R}$, reduced to percentages, $\frac{200D}{R}$. The measurement of brightness in this way is open to much objection. But the percentages may be regarded in another light, which is equally useful. The numbers given are twice the percentages of the changes made in distance, in order to produce the equivalent changes in distinctness. From this point of view the measurements are independent of any assumption, and comparisons can be made among them.

In accordance with this the formula $\frac{200D}{R}$ will be used in every case, though it is inaccurate for measuring increases of brightness when large changes take place.

By means of the curves drawn from these observations, we can form some idea of the behaviour of the retina around the yellow spot. It is only this region which is of importance in this subject.

It is desirable that observations of this class should be made with more than one individual. For this reason, two series of observations have been made—one by Mr. Quinlan, and the other by myself. And here I desire to express my obligations to Mr. Quinlan, who has been of much service to me in the course of the investigation, in enabling me to test and confirm observations again and again.

It has been thought sufficient merely to give those curves which map out the sensitiveness of the retina over the area observed. It is well to mention that observations taken with an interval of two months were concordant. It is remarkable with how much precision readings can be taken: the curves are drawn directly from observations, but it was only after some practice that such precision was acquired. Fig. 1 represents Mr. Quinlan's observations; fig. 2, my own.

The numbers on the curves are a rough indication of the change of

sensitiveness, taking the sensitiveness of the fovea as 100. They are got from the last observation mentioned. Though these numbers are only rough estimations, yet they are precise enough to indicate a marked difference in the retinas of the two observers. The variations in fig. 1 are much larger than those for the same curve in fig. 2. This great difference seemed so extraordinary that the eyes were tested again and again in various ways with similar results.

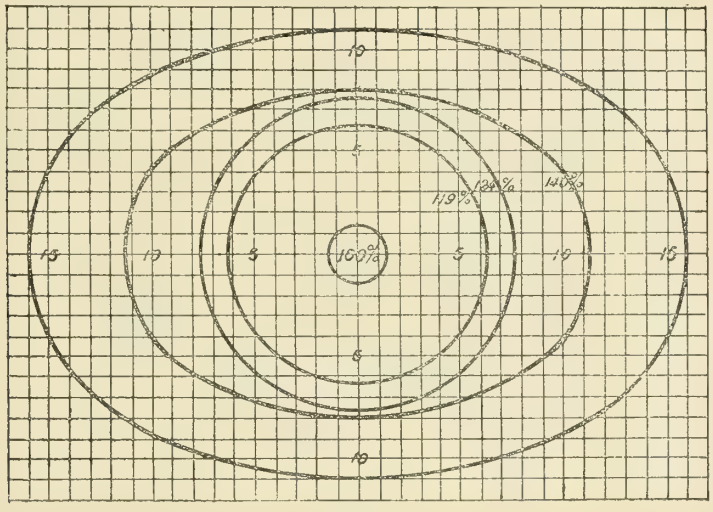

FIG. 1.

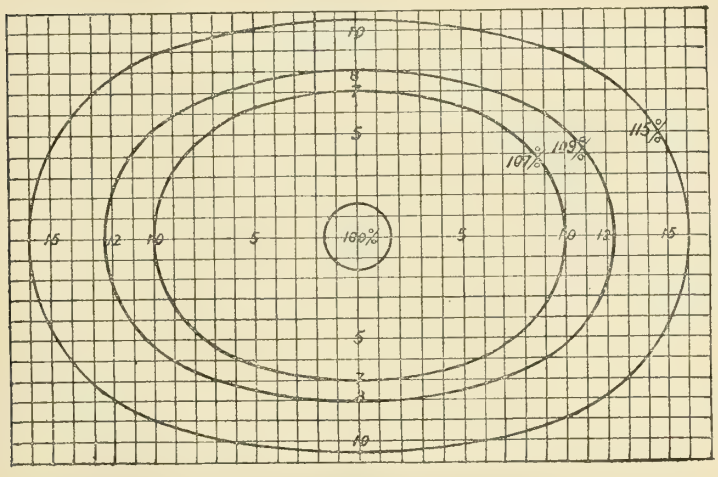

FIG. 2.

This greater variation in sensitiveness, combined with the greater distance of observation in Mr. Quinlan's case, enabled observations of his eyes to be taken very easily.

Mr. Quinlan's sight is normal, and his distance of observation ranged from

50 to 60 cm. in this experiment. But as I am a little short-sighted, my range was usually from 30 to 40 cm. with the same object. This difference in distance of observation has no bearing on the results, because curves got by Mr. Quinlan at 30 cm. did not differ from those got at 50 cm. in their general features. But it is evidently more advantageous to get readings at the greatest distance which suits the observer.

These curves, besides affording some definite information about the observer's eye, are also interesting from a physiological point of view. The contrast between the inner and outer curves in fig. 1 is striking. It is intimately connected with the physiology of the eye. The yellow spot is, as is well known, an oval with its long axis horizontal. This axis is usually about 2 mm. long, and the short axis is stated to be about two-thirds of this. It will be noted that the innermost oval closely corresponds to these dimensions. In fact, this curve was very often obtained in the investigations. It would seem, then, the boundary of the yellow spot is the transition curve from circles to ovals. I have got no inside curves in my own case, owing to the causes mentioned above, but observations in Mr. Quinlan's eyes confirm this fact.

Again, physiologists ascribe the increased sensitiveness to the recovery of the visual purple in the dark. The distribution of visual purple increases outwards from the fovea centralis, and there is none in the fovea centralis. If this be the case, then these curves indicate how the density of visual purple increases. As long as we are inside the yellow spot, the change of distribution is symmetrical with respect to the circular boundary of the fovea; while outside, the distribution is symmetrical with respect to the oval boundary of the yellow spot itself. I am not aware that the retina has ever been examined from this point of view.

It should be mentioned, in connection with this experiment, that resting the eyes or any ordinary stimulation of the retina fails to cause any change in the appearance of the slits as long as their image falls on the fovea. The question how fatigue or stimulation affects the other parts of the retina is too complicated and difficult to be investigated in the manner described.

The curves indicate the extent of the change in the appearance of an object by the displacement of its image on the retina. To an inexperienced person, a phosphorescent screen appears to be going through a series of bewildering variations. The greater part of these, if not all, are due to motions of the eye. They gradually diminish, as practice gives greater control over the eye.

The principal facts to be noted are that the most common motions of the eye are horizontal; but, as a set-off, a vertical movement in general causes a greater change than an equal horizontal movement. It is an important question, then, for any observer to know how to get these motions under control, and what objects

are most suitable for these observations. I shall, therefore, mention a few points which seem worthy of special emphasis.

An object whose image is not larger than the area of the fovea is occasionally the only one suitable for observation, but it needs great care in its use, as one needs to keep the eyes consciously fixed on it. It is extremely easy to imagine that the gaze is fixed directly on such an object; whereas its image is displaced over the retina, and consequently appears brighter than when on the fovea. Lummer has already emphasized this point, but I was surprised at the ease of self-deception on this matter. In fact, it was the greatest difficulty in getting the curves of equi-sensitiveness, as it was necessary that one should keep conscious control over the eyes.

A larger object is less trying to observe, but it gives the eye greater freedom of motion, causing much apparent variation. The means adopted to avoid this may seem artificial, but they correspond to the peculiar character of the dark-adapted retina. In order to observe small changes when the screens were bright, the device of slits was used. This device makes the variation caused by eye-movements quite plain. But after some practice the appearance of the object can be kept constant. After many trials a diaphragm, easy to observe and sensitive to small variations in brightness, which enabled the observer to control these motions of the eye, was adopted. The particular dimensions of such a diaphragm would depend on the individual. The general form used resembled a capital H, with an additional vertical line down the middle. This gives three slits crossed by a horizontal band, which served as a spot on which to keep the eye fixed. In one case the length was about 5 cm., the broad horizontal band 1 cm. deep, and the slits were 3 mm. wide, separated by an interval of 4 mm. This diaphragm was used with screens which had the brightness acquired in diffused daylight. The distance of observations of screen and diaphragm was about 80 cm. in Mr. Quinlan's case, and about 50 cm. in my own. At these respective distances the slits became blurred, and slight changes in brightness caused them to re-appear again.

The object was kept of constant appearance by keeping the eyes directed towards the central band, and correcting any changes which might occur by consciously re-directing the eye towards it. Any change which could not be so corrected cannot be a change due to any displacement on the retina. In special cases this correction may be assisted by the peculiar change in appearance caused by such displacements.

It is necessary to mention some other subjective causes which may disturb the appearance of the object when its image on the retina is large. The screen apparently increases in brightness (1) by resting that part of the retina on which

the image falls, by closing the eyes or otherwise; (2) by any muscular agitation of the eyes. These changes take some time to die away, and, it can be ascertained, they are not due to a mere displacement on the retina. They do not seem to be present, as already noted, when the image falls within the fovea centralis. In some tentative measurements they amounted to about 7 per cent. Such effects depend so much on the observer and his state of health that it is not deemed advisable to do more than mention their existence until they have been tested under all kinds of conditions.

II.

The effect described by Blondlot could be observed by this screen and diaphragm in such a way that subjective variations were eliminated. Substances under strain were found the most suitable sources of N-rays. Wood or cork compressed, tempered steel, and unannealed glass were used for this purpose. As other more intense sources emit heat in addition to N-rays, their use could not be satisfactory.

Some estimations of the magnitude of the effect perceived were taken. The screen and diaphragm were placed at such a distance that the pattern was rather indistinct. When a source of N-rays was brought up behind the screen, the pattern came out more clearly. The observer now noted the appearance of the screen. When it had returned to its initial condition after the removal of the N-rays, it was moved towards the observer until the decrease in distance had produced a change in the screen similar to that produced by the strained substance. The two positions of the screen were marked and afterwards measured, and the increase of brightness deduced as before from the formula $\frac{200D}{R}$.

The numbers obtained from different sources in this way for the same screen are given below.

Source.	Increase of brightness.
Unannealed Glass, . . .	10 per cent.
Compressed Cork, ...	9 ,,
Sounding Tuning-Fork, ...	8 ,,
Silent Tuning-Fork, ...	3 ,,

Some idea of the effect of heat on the screen was got by using as a source of heat a column of water 5 cm. long in a test tube. When the water was about 60° C. it caused an increase of brightness of 5 per cent., while with water at 20° C. there was no noticeable change in the screen. In order to test the observer, the

minimum change he could detect was measured. This was done by finding the least distance it was necessary to move the screen forward in order that a change could be clearly perceived. In the case of this experiment the minimum change detected was 3 per cent., so that the heat effect of water at 20° must be less than this.

The effects due to strained substances, then, can scarcely be assigned to heat, as water at 20° certainly did not cause a greater increase than 3 per cent., and the increase of brightness produced by water at 60 per cent. was much smaller than most of the effects due to strained substances. As the observations are free from subjective variations, we can only assign the effects due to substances under strain to some other objective cause than heat. The effects look large on paper, but in reality are not so. An ordinary person without any previous training found great difficulty in detecting small changes in this manner. The screen never seemed to be constant in appearance.

These subjective variations obscured real changes of brightness, and their effect is clearly perceived in the minimum change which could be detected. The most common minimum for an untrained observer was about 10 per cent.; with a little more practice the minimum might be brought down to 6 per cent. Using the screen alone without any diaphragm raised the minimum change which was visible. It is evident that the effects described above would be invisible to a person whose minimum was 10 per cent., while a person whose minimum was 6 per cent. should see the effect, but in this latter case the increase of brightness is so close to the minimum that it would be difficult for the observer to definitely assure himself of any increase at all. It is not surprising, then, that so many have failed to see the effects. The possibility of observing a small change depends on training which enables one to get rid of subjective variations, and keep the appearance of the object constant, so that a small change in brightness can be observed. I wish to add that it was only after much practice that Mr. Quinlan and myself were able to assure ourselves of the changes I have mentioned.

The reason why so many have failed to confirm Blondlot's experiments is that the effects to be observed were only just within the range of visibility of all except those of considerable experience. The scientists in France who have confirmed Blondlot's work are mostly those within his own circle, and others who have done work on photometry before, and consequently have got the necessary experience.

This state of affairs is not satisfactory. As the existence of N-rays is only shown by their property of increasing the brightness of a phosphorescent screen, there is great need of knowing how this increase may be made manifest to any person without trouble, and in such a way that the effect can only be assigned to N-rays.

III.

I shall conclude my paper with a description of such an experiment. The observations mentioned above were taken with screens which had been exposed to daylight. It is probable that most observations have been hitherto made under these conditions. Such screens are comparatively bright, and N-rays cause, as we have seen, only small changes in them.

But if we take very dim screens, the N-ray effect is relatively much larger. When the phosphorescent screens are kept for a long time in total darkness, their phosphorescence disappears. If they are now exposed from one to two minutes to gaslight, at a distance of about 6 feet, they acquire a slight phosphorescence, which decreases quickly for a short time. After the initial rapid decrease is over they are suitable for observation. In such screens the outline alone suffices for the guiding indistinctness. In the experiment, it is well to have another screen which has gone through the same treatment. These two screens are placed so that the outline is slightly indistinct. This occurs at a distance of 30 to 40 cm. In this state the two screens can be adjusted so that the indistinctness of each screen is the same. It is usually stated that objects appear of equal brightness at all distances. If the screens were initially of different brightness, it would then be impossible to make them appear of equal brightness. But in the case above we are solely paying attention to indistinctness—in fact, it is well to neglect the intrinsic brightness of the two screens altogether, as it is liable to confuse one. However, for convenience, the screens will be called equally bright when they appear equally indistinct.

The experiment with N-rays was carried out in this way. The observer placed the screens at such a distance that the indistinctness of outline could be easily observed. One screen was fixed in position, and the observer was asked to move the second one until it seemed as bright as the first. When this had been done, the position of the screen was noted; several comparisons were made in this way. After about half a dozen attempts the observer could generally compare the indistinctness with a certain amount of precision.

If R were the distance of the movable screen from the observer when its indistinctness is equal to that of the fixed screen, the precision generally attained was such that the various values of R did not differ by more than 1 or 2 cm. When such precision was arrived at, the observer could be quite sure the screens were equally indistinct. If some source of N-rays was then brought up behind either screen, the balance of brightness was disturbed after a short time. The observer could no longer say one screen was as bright as the other. He would be

certain there was a difference. Finally, the screen, under the influence of N-rays, could be unhesitatingly pronounced the brighter.

Some idea of the difference in the screens was got by asking the observer to re-adjust the balance between the screens. The change of distance required was relatively large. The experiment can be subjected to the most rigid tests. The observer may be ignorant of what screen is being influenced by the rays; but he will have no doubt which screen is the brighter. The observer was usually tested in this way.

But unless some precision is attained in comparing indistinctness, it would be futile to proceed with the latter part of the experiment. No difficulty has been found in getting this precision with any person of normal sight, and several experiments made with such persons have been uniformly successful. After some practice, a person of defective vision—short-sighted, for example—can acquire the necessary precision; and when he has done so, there is no difficulty in repeating the experiment.

I shall now give some estimations of the N-ray effect under these conditions made by a trained observer. When the screens are equally indistinct, their intensities are assumed to follow the law of inverse squares, which I have already used in the course of this paper. After some training, the two screens can be compared with great exactness. One screen will be moved back into almost the same position time after time. From many observations of this class, it seemed that two screens could be compared within 3 per cent.

The N-ray effect was deduced from the change of distance required to restore the balance after it had been disturbed, combined with the distance of the screen. The same formula $\frac{200D}{R}$ was also used to reduce these observations. The numbers obtained varied from 20 per cent. to 40 per cent., according to the source used and the conditions of the screen.

Another way of looking at the effect is to consider the change of distance required to restore the balance of the screens. This varied from one-sixth to one-tenth of the total distance. For example, if the movable screen is at a distance of 40 cm., and N-rays influence the second screen, the first screen will be moved forward from 4 to 7 cm. to restore the balance of brightness.

I have tried to describe how these effects may be seen, but there are many things which cannot be communicated in a paper. I am sure that any person who takes a little patience in working with the screens in the manner mentioned will have no difficulty in repeating the experiments.

So far I have only dealt with the simplest property of N-rays—that of increasing the brightness of a phosphorescent screen. I am satisfied that this effect

can be observed under conditions which admit of no subjective or heat explanation. But I have not succeeded in observing many other peculiar properties of N-rays under such satisfactory conditions, with a single exception. In carrying out an experiment with dim screens, an initial decrease of brightness was remarked in the screen under the influence of N-rays in certain cases, which was soon succeeded by an increase of brightness in the same screen. The effect corresponds to Blondlot's N_1-rays, but it does not lend itself to measurement, nor can its existence be rigorously tested, as it is only a temporary effect.

In conclusion, I desire to express my great obligations to Professor M'Clelland, who suggested I should inquire into this subject, and gave me much encouragement and advice during the progress of the investigation. I also wish to mention that I am greatly indebted to Professor Coffey for enlightening me on physiological questions.

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ON THE EXTRACTION OF GLUCINUM FROM BERYL.

By JAMES HOLMS POLLOK, B.Sc.

[Read, May 17, 1904.]

I. Introduction.

In the course of a recent research on the heat of formation of Glucinum Chloride, I extracted a considerable quantity of glucina from beryl; then prepared and carefully analysed the hydrate, basic carbonate, sulphate, and anhydrous chloride; and finally extracted the metal. During the progress of this work many modifications were introduced in the method of extraction that greatly improved the process, and made it much more easy and convenient in practice, and gave a product almost free from iron and alumina. Every effort was made to make the glucina as pure as possible; and the analyses of the various preparations were very carefully done, so that the fact that they do not agree with theory points to the desirability of farther investigation on the composition of beryl and the atomic weight or equivalent of glucinum.

I will first give a short synopsis of the work done by others, and afterwards the results of my own experiments.

II. Historical Account.

The distinguished crystallographer Haüy, having noted the identity of form of beryl and emerald, requested Vauquelin* to make a careful analysis of the former. This Vauquelin proceeded to do, fusing the beryl with three times its weight of caustic potash, and separating the silica in the usual way. He then precipitated the earths by carbonate of potash, digested the precipitate with caustic potash, and noted that, from the caustic potash solution, a portion amounting to nine per

* Ann. Chim., [1], vol. xxvi., 1798, p. 155.

cent. was subsequently reprecipitated. The investigation of this phenomenon, of reprecipitation from caustic potash, finally led to the discovery of the new earth glucina.

In the year 1843 Awdejew* made an analysis of the chloride and sulphate, and thence deduced for the equivalent of glucinum the figure 4·64.

The hydrate, basic carbonate, and sulphate were carefully analysed by Weeren in 1854,† the glucina being purified by fractional solution in ammonium carbonate, to remove the last traces of alumina.

Debray,‡ in 1855, again analysed the sulphate, by ignition in a platinum crucible, alone, and with lime, from which he deduced both the water and sulphuric acid. He also analysed the basic carbonate and a number of other compounds.

A large number of methods of decomposing beryl, and separating glucina, were tried by Joy§ in 1863. The method finally adopted was to fuse with alkaline carbonates, decompose with sulphuric acid, crystallize out most of the alum, and separate the glucina from the mother-liquors by pouring it into an excess of ammonium carbonate, then to boil out the glucina.

The paper on the specific heat of glucinum by Reynolds‖ in 1877 was the beginning of a brilliant series of papers, by various authors, on the atomicity of glucinum. Those by Nilson and Pettersson,¶ and Humpidge,** on the specific heat of the metal, and by Nilson and Pettersson†† on the vapour density of the chloride, are perhaps the most important. Nilson and Pettersson long maintained that glucinum was trivalent; and it is interesting to note how their own paper, on the vapour density of the chloride, finally set the matter at rest, and proved that glucinum was divalent.

Marignac‡‡ investigated the properties and composition of the fluoride and double fluorides of glucinum in 1873. In 1890 Kruss and Moraht§§ contributed a long paper on the extraction of glucina and preparation of glucinum by reduction of the fluoride with sodium, and they point out the anomalous behaviour of some substance contained in the ammonium sulphide precipitate.

The analytical side of the question has been dealt with by C. Rössler,‖‖ in his paper on the separation of aluminium and glucinum by phosphate of ammonia.

* Ann. Chim. Phys., [3], vol. vii., 1843, p. 155.
† Pogg. Ann., vol. xcii., 1854, p. 91.
‡ Ann. Chim. Phys., [3], vol. xliv., 1855, p. 5.
§ Amer. Journ. Sci., [2], vol. xxxvi., 1863, p. 83.
‖ Phil. Mag., [5], vol. iii., 1877, p. 38.
¶ Compt. Rend., vol. lxxxvi., 1878, p. 823.
** Roy. Soc. Proc., vol. xxxviii., 1885, p. 188.
†† Compt. Rend., vol. xcviii., 1884, p. 988.
‡‡ Ann. Chim. Phys., [4], vol. xxx., 1873, p. 45.
§§ Ber. Deutsch. Chem. Gesell., Jahrg. xxiii., 1890, p. 727*a*, p. 2552*b*.
‖‖ Zeitsch. Anal. Chem., vol. xvii., 1878, p. 148.

Zimmerman* investigated the original method of separating aluminia and glucina, by diluting and boiling the potash solution of the oxides. Atkinson and Smith,† in 1895, published a method for the separation of iron and glucinum by nitroso β naphthol after the method of von Knorre. Havens‡ shows that aluminium and glucinum may be accurately and quantitatively separated, by adding ether to the mixed chlorides, and saturating with hydrochloric acid gas; all the aluminium chloride precipitating, and none of the glucinum chloride. Hartley§ has investigated the spectrum of glucinum. Lebeau‖ has lately described an excellent method of making glucinum, by the electrolitic decomposition of the fused double fluoride of glucinum and sodium; also the reduction of the oxide, when mixed with another metal, or metallic oxide, in the electric furnace.¶ The same author** describes the formation of the carbide and iodide of glucinum.

Pettersson†† gives the heat of neutralization of glucinum fluoride as $Gl(OH)_2$, 2HF, Aq = 19,683 cal.; and Carnelly‡‡ has given the melting-point of glucinum chloride as between 585°–617°, and maintains the accuracy of these figures, though they are called in question by Nilson and Pettersson, who point out that glucinum chloride boils at 520°. Many other authors have contributed to the literature of glucinum; but their papers have no direct bearing on the subject of the present inquiry; and even the shortest reference to each would entail a long dissertation.

III. Analysis of Beryl.

Ten pounds weight of beryl, from Limoges, was procured from Messrs. Harrington Bros., of London. It was of a pale green colour, opaque, and in plates or sheets, not showing any signs of hexagonal crystals; it seemed of uniform character, and not contaminated with rock, or other minerals. The whole quantity was ground up, mixed, sampled, and a fine sample carefully ground for analysis. The analysis was performed by fusion with alkaline carbonates, separation of silica, precipitation with sulphuretted hydrogen, ammonia, sulphide of ammonia, carbonate of ammonia, and phosphate of soda; and the alkalies were estimated by treatment with hydrofluoric acid; all after the usual manner. The iron was separated from the alumina and glucina by caustic soda, redissolved in hydrochloric acid, and reprecipitated by ammonia. The caustic soda solution was acidified with hydrochloric acid; and the glucina and alumina precipitated by

* Zeitsch. Anal. Chem., vol. xxvii., p. 61.
† Journ. Amer. Chem. Soc., vol. xvii., 1895, p. 688.
‡ Amer. Journ. Sci., [4], vol. iv., 1897, p. 111.
§ Chem. Soc. Journ., vol. xliii., 1883, p. 316.
‖ Compt. Rend., vol. cxxvi, 1898, p. 744.
¶ Compt. Rend., vol. cxxv., 1897, p. 1172.
** Compt. Rend., vol. cxxi., 1895, p. 496.
†† Zeitsch. Phys. Chem., vol. v., 1890, p. 259.
‡‡ Ber. Deutsch. Chem. Gesell., Jahrg. xvii., 1884, p. 1357.

ammonia, well boiled, filtered, and washed; then redissolved in the least possible quantity of hydrochloric acid just precipitated with ammonium sesquicarbonate, and then poured into a considerable excess of sesquicarbonate of ammonia, and allowed to stand overnight; and the precipitated alumina was well washed, then dried, burned, and weighed. The solution was boiled up to separate the glucina, and the resulting precipitate washed, dried, burned, and weighed. The results of a number of fairly concordant analyses were as follows:—

Silica, ...	64·41
Alumina, ...	18·82
Glucina, ...	11·65
Lime, ...	·65
Copper, ...	a trace
Ferrous oxide, ...	1·46
Magnesia, ...	·02
Nickel and zinc oxides, ...	·74
Combined water, ...	1·22
Alkaline chlorides, ...	·68
	99·65

There was a slight precipitate with sulphuretted hydrogen, that proved to be for the most part copper; and the sulphide of ammonia precipitate, recorded as nickel and zinc, was of a somewhat doubtful character, appearing to contain something in addition to these elements.

Subsequently a second lot of 20 lb. of similar beryl was procured, but was not analysed.

IV. Extraction of Glucina.

After a number of trials, I found that beryl is very easily decomposed by fusion with caustic soda, and that a comparatively small quantity of this flux is sufficient to effect complete decomposition, so that it is much more convenient to use than the mixed carbonates of potash and soda, especially when working on a large quantity.

In one experiment 2000 grams of ground beryl was fused with its own weight of caustic soda, in a large Salamander crucible. It gave a good tranquil fusion in about one hour, the contents of the crucible being perfectly liquid and pouring well. When cold the fused mass was broken up, and ground to pass a sixty-mesh screen, then put in a large porcelain jar holding about four gallons, and treated with excess of strong hydrochloric acid and well stirred up, the heat generated by the reaction bringing the whole mass to the boiling-point, without the necessity of external heat. Next day the liquid was decanted off, more hydrochloric acid

and water added, and the whole boiled up by steam, allowed to settle, the solution siphoned off, and the silica thrown on a linen filter and well washed. The solution was now precipitated by ammonia, and the very bulky precipitate of iron, alumina, and glucina, filtered off and dissolved in hydrochloric acid, and the solution saturated with hydrochloric acid gas, which precipitates nearly all the aluminium as chloride, but leaves the whole of the iron and glucina in solution. Any soluble silica present comes down as a gelatinous precipitate before the precipitation of the aluminium chloride begins. The precipitated aluminium chloride was filtered off through glass wool, by a vacuum pump, and the precipitate well washed with a saturated solution of hydrochloric acid. The precipitated aluminium chloride was pure white, crystalline, and easily filtered. On boiling off the hydrochloric acid from the solution and evaporating down, a deep reddish-yellow syrupy solution was obtained, containing the iron and glucinum as chlorides. This solution was added in quantities of 100 cc. at a time to two litres of saturated solution of ammonium carbonate, and well shaken up in Winchester quarts. At first a heavy curdy precipitate came down, which, on shaking, redissolved completely to a perfectly clear solution; in about five minutes this became cloudy, and in ten minutes quite a heavy precipitate came down; this very curious phenomenon being exactly repeated in all the Winchesters used. The time of appearance of the first cloudiness was very regularly five minutes, the precipitation being apparently complete in ten minutes.

To ascertain the effect of varying the proportion of carbonate of ammonia, and its degree of dilution, a series of experiments were made, using measured quantities of saturated solution of glucinum chloride, carbonate of ammonia, and water. The mixture was well shaken up until it became perfectly clear, the time then noted, and the liquid left at rest until the first appearance of cloudiness, when the time was again noted; then, so far as possible, the time of apparent complete precipitation was noted; and the following results were obtained:—

Saturated Solution of $GlCl_2$, &c.	Saturated Solution of Am. Carb.	Water added.	On shaking, got—	First Cloudiness in—	Complete Precipitation in—
2 cc.	20 cc.	0	Milky	—	2′
1 cc.	20 cc.	0	Clear	6′	11′
1 cc.	20 cc.	20 cc.	Clear	8′	13′
1 cc.	60 cc.	0	Clear	20′	35′

In the first experiment there was not sufficient carbonate of ammonia for complete solution, between the second and third experiments there was little

difference; and with a great excess of carbonate of ammonia, a much longer time was taken for the precipitate to form. This precipitate, on examination, proved to consist mostly of alumina, with a little iron and glucina; about 80 grams of it were collected, and it will be examined at some future date.

The clear solution of glucina in carbonate of ammonia was now treated with a few drops of sulphide of ammonium, and allowed to stand for two days. The black precipitate proved to be for the most part iron, zinc, and nickel, but another substance appeared to be present; the quantity, however, was too minute to admit of any satisfactory conclusions being drawn regarding it. The peculiarity of this sulphide of ammonia precipitate was also observed by Kruss and Moraht. The carbonate of ammonia solution of glucina, now free from silica, alumina, and iron, was boiled up and evaporated to about half its bulk, the glucina precipitating as a beautiful white basic carbonate of glucina, containing nothing but a small quantity of alumina. The precipitate was dried and bottled for use, and will be referred to as crude basic carbonate of glucina.

A second quantity of 2000 grams was similarly decomposed with caustic soda; but the ground mass was treated with diluted sulphuric acid, and well boiled up by steam; then filtered from the silica, and the solution precipitated by ammonia, the precipitate dissolved in sulphuric acid, and potassium sulphate added to the solution, which was then evaporated down to the crystallizing point, and allowed to cool. Next day the crystals of alum were drained off, the solution re-evaporated and the process repeated until a portion of the mother-liquor, when poured into an excess of carbonate of ammonia, gave a precipitate that completely redissolved on shaking up, though the phenomenon of reprecipitation on standing was again observed. This strong solution of glucinum sulphate was poured into saturated solution of carbonate of ammonia in quantities of 100 cc. to two litres of carbonate of ammonia, and well shaken up in Winchester quarts, dissolving clear, or nearly so, and afterwards giving a copious precipitate, as in the previous experiment. After filtering off the reprecipitated portion, the solution was treated with sulphide of ammonia, and the black precipitate of iron, and whatever else might be there, removed, and the solution boiled to separate the basic carbonate of glucina.

In a third experiment about 4000 grams of beryl were fused as before, then treated with an excess of sulphuric acid, and the products systematically crystallized out. The excess of sodium sulphate crystallized first, then the soda alum, and finally the glucinum sulphate. The glucina was separated from the crude sulphate, and mother-liquors, as basic carbonate, and purified as before.

In a fourth experiment about 4000 grams were fused with caustic soda, and treated with sulphuric acid as before; but after most of the alum had crystallized out, the solution was treated with its own volume of alcohol, which

precipitated the remainder of the alum, leaving only the glucina and iron in solution. After distilling off the alcohol, the iron and glucina were separated as before.

A preliminary experiment on about 1000 grams of beryl also yielded a quantity of crude glucinum basic carbonate; but as it was of an experimental character, and substantially the same as the first process described, it is not necessary to give details farther than to say that the oxides were precipitated in two fractions by ammonia; and nothing was gained by this, as the alumina and glucina came down in much the same ratio in both fractions. Fractional precipitation with carbonate of soda also failed to separate the two bases. Probably the best means of extraction would be to fuse the beryl with caustic potash in place of soda, then to treat direct with sulphuric acid, filter off the silica, and crystallize out the alum, and then use carbonate of ammonia, thus avoiding the precipitation by ammonia. This is not easily done in the case of fusion with caustic soda, owing to the great solubility of soda alum.

There was a good deal of loss in the various processes of extraction, part of the glucina evidently crystallizing out with the alum and aluminium chloride, for the yield was below that shown by analysis.

V. Quantitative Separation of Aluminium and Glucinum.

None of the methods described for the separation of glucinum and aluminium were found very convenient in practice. Fusion with acid potassium fluoride and solution with hydrofluoric acid, as described in Fresenius, are said to give the most accurate and reliable results. I found, however, that the following modification of this process is much more convenient in practice, especially as it can be applied direct to a mixed precipitate of alumina and glucina, in the platinum crucible in which it has been ignited and weighed.

To one gram or less of the ignited oxides, or dry hydrates, or carbonates, add 10 cc. of hydrofluoric acid; then dilute in a platinum basin by the addition of not less than 250 cc. of distilled water, and add 20 cc. of a strong solution of acid fluoride of potassium. Any alumina present at once precipitates as the double fluoride $AlF_3 2KF$; and is filtered off in the cold, using a vulcanite or platinum funnel. If one is short of platinum basins, a beaker lined with a little paraffin wax will do to catch the filtrate and washings. After washing, the precipitate is dried, removed from the paper, the paper burned, and then the precipitate added to the crucible and heated to redness for a moment, when fairly accurate results are obtained. If rigorous accuracy is required, the precipitate must be decomposed by strong sulphuric acid, heated until fumes of sulphuric acid are coming off freely to drive off all the hydrofluoric acid, then dissolved, and the alumina

precipitated by ammonia, and weighed as usual. If required, the glucina is obtained from the filtrate by evaporation in a platinum basin, decomposition with sulphuric acid and potassium bisulphate, then dissolved, and the solution precipitated by ammonia.

Berzelius first pointed out that the double fluoride of aluminium and potassium, obtained by adding a solution of acid potassium fluoride to a solution of alumina in hydrofluoric acid, has the composition $AlF_3 2KF$, which composition my analysis entirely confirms; he also investigated the precipitate obtained by adding the solution of alumina in hydrofluoric acid to the potassium acid fluoride, when a different and more gelatinous and gummy precipitate is obtained, having the composition $AlF_3 3KF$; there is, however, no danger of forming this precipitate if the analysis is conducted as described.

If the dilution is not of the degree specified, glucinum double fluoride will also be precipitated, as it is not very soluble, and requires at least fifty times its weight of water for solution. For this reason, the method, though convenient for analysis, is not at all convenient for preparation of a large quantity of glucina; but if the hydrate or carbonate of glucina be nearly free from alumina, it may be made perfectly pure by dissolving in hydrofluoric acid, and then adding a quantity of potassium acid fluoride, just sufficient to precipitate all the alumina present, or a little more, when all the alumina will come down, and some glucina with it. This avoids the necessity of greatly diluting the solutions, and the use of very large platinum vessels. If excess of potassium acid fluoride were added to a strong solution of glucina, most of the glucina would precipitate, as well as the alumina. After the removal of the precipitate containing the double fluoride, the solution is evaporated down to a gummy consistency, decomposed with sulphuric acid, heated until dense fumes come off, then dissolved, and the glucina separated as hydrate or basic carbonate, by ammonia or carbonate of ammonia. The method, though inconvenient, is the only one I am acquainted with, by which one can obtain chemically-pure glucina, free from every trace of alumina.

VI. Glucinum Basic Carbonate.

To purify the basic carbonate of glucinum, I decided to use the method of fractional solution in ammonium carbonate described by Weeren in his researches; that is, to add a quantity of ammonium carbonate less than sufficient to dissolve the basic carbonate, as it is said that under these conditions only pure glucinum carbonate dissolves, and any alumina present remains undissolved.

To ascertain the solubility of the glucinum basic carbonate in ammonium carbonate, for the purposes of the experiment, six grams of freshly precipitated

dry carbonate were added to 30 cc. of a saturated solution of commercial sesquicarbonate of ammonia, and well shaken for two days; they were then filtered, and the solution boiled down, to reprecipitate the dissolved carbonate, which was then filtered off, dried, burned, and weighed, giving ·6656 grams of oxide, or about 58 grams of glucinum basic carbonate, equivalent to 22 grams of ignited oxide, dissolved per litre of saturated solution of ammonium sesquicarbonate.

A saturated solution of sesquicarbonate of ammonia was now prepared by dissolving 167 grams of the salt in 1 litre of water; this gave about 1200 cc. of solution, and of this 1000 cc. were taken, and 80 grams of dry basic carbonate added, and well shaken up at frequent intervals for a week; it was noted that there was a continual evolution of carbon dioxide as the solution progressed. At the end of the week the undissolved residue was filtered off, and the solution boiled down. On boiling, the basic carbonate of glucina does not at once begin to precipitate, there being little or no precipitate, until a certain proportion of the ammonium carbonate is decomposed, and then the glucinum basic carbonate is rapidly precipitated, and it is practically all precipitated, when the solution is evaporated to half its bulk, or when a thermometer indicates that the temperature has reached 100°. After filtering off the reprecipitated glucinum basic carbonate, the residual liquors were evaporated to dryness and ignited, and gave only ·27 grams. The fraction that dissolved and reprecipitated weighed, on drying, 66 grams. The undissolved fraction weighed 10 grams.

An analysis of the basic carbonate of beryllia gave:—

	Per cent.
Glucina, ...	37·9 ÷ 25 = 1·5
Carbon dioxide, ...	22·9 ÷ 44 = ·5
Water, ...	42·0 ÷ 18 = 2·2

corresponding to the formula $GlCO_3 2GlH_2O_2 2H_2O$; but the composition is not really very constant, the water being liable to vary from two to three molecules, and even the ratio of the carbonate to the hydrate is subject to some variation. In another experiment, where the first portion to be precipitated was filtered off, and the boiling then continued and the second portion kept separate, an analysis gave:—

	First Fraction. (Per cent.)	Second Fraction. (Per cent.)
Glucina, ...	34·4	40·5
Carbon dioxide, ...	23·6	21·0
Water, ...	42·0	38·5

Evidently the first portions to come down contain more carbonate, and the latter portions more hydrate, and all rather more water, than the formula given.

To test the purified basic carbonate of glucina for alumina, 2 grams were dissolved in 10 cc. of hydrofluoric acid, and 250 cc. of water added, and then 5 cc. of a 20 per cent. solution of acid potassium fluoride, and the precipitated double fluoride of potassium and aluminium filtered off, washed, dried, burned, and weighed. The result gave ·022 grams of precipitate, corresponding to ·28 per cent. of alumina. The undissolved residue when tested in the same way gave 4·49 per cent. of alumina; and the hydrofluoric acid solution, when decomposed by sulphuric acid, and precipitated by ammonia, gave ·844 grams of GlO, corresponding to 42·2 per cent. of glucina.

VII. Glucinum Hydrate.

A quantity of hydrate was now prepared by dissolving the pure basic carbonate in hydrochloric acid, washing, and drying at 100°C. The precipitate was very bulky and gelatinous, drying to a white, and very light and bulky, amorphous powder. On ignition, two different samples gave—

1·511 grams dry hydrate gave ·802 gram = 53·08 per cent. GlO.
1·419 ,, ,, ,, ·770 ,, = 54·30 ,, GlO.

This corresponds to the formula $4GlO5H_2O$.

VIII. Glucinum Sulphate.

A quantity of the purified basic carbonate was now dissolved in dilute sulphuric acid, filtered and left to crystallize over sulphuric acid in the air-pump, and gave a good crop of large crystals. From the mother-liquor, a second crop was obtained. The first crop gave, on ignition of 1 gram, ·146 and ·145 gram of glucina, equal to 14·55 per cent. GlO. The formula of the sulphate is $GlSO_44H_2O$, and for this theory requires 14·17 per cent. GlO.

An analysis of the second crop by solution and precipitation gave:—

	Per cent.	Theory. Per cent.
Glucina, ...	14·34	14·17
Sulphur trioxide, ...	46·60	45·17
Water, ...	39·06	40·66
		100·00

The first crop was in much finer crystals than the second; and it was unfortunate that the complete analysis was not done on this portion. Two or three of the largest crystals were placed on a watch-glass, and put in a dessicator over sulphuric acid, and weighed from day to day. The first day there was a very slight loss of adhering moisture, but afterwards there was no further loss of

weight, nor was the lustre of the faces of the crystals at all dimmed after a fortnight, showing that the crystals were quite permanent in dry air. The sulphate is not soluble in anhydrous alcohol, but dissolves freely in 50 per cent. alcohol. It decomposes to oxide when ignited for some time over the blowpipe; but a good heat must be maintained for about twenty minutes to ensure complete decomposition. The heat of an ordinary Bunsen is not nearly sufficient.

Many other samples of crystallized sulphate were examined and gave on ignition from 14·00 per cent. to 14·20 per cent. of oxide, which would agree exactly with the accepted atomic weight of glucinum: but one or two samples gave an even lower figure, which is somewhat remarkable, as they were well crystallized; it was noticed that the lowest figures were always obtained from the glucina prepared by the alcoholic method, and from that portion most soluble in moderately strong alcohol. Again, though the oxide as estimated by ignition seemed as a rule to bear out the accepted value of the atomic weight when complete analyses were made, the ratio of the base to the acid seemed to be subject to considerable variation. In the last analysis the acid is too high, but in another sample of very beautiful crystals, prepared from the least soluble portion of the glucina obtained by the alcoholic method, the following results were obtained.

Glucina, ...	14·20
Sulphur trioxide, ...	43·39
Water, ...	42·41
	100·00

IX. Anhydrous Chloride of Glucinum.

A number of preliminary experiments showed that the best method of making anhydrous chloride of glucinum was to mix the oxide with four times its own weight of sugar, and char in a Hessian crucible at a dull red heat; then to place the intimate mixture of carbon and glucinum thus formed in a porcelain tube, maintained at a bright red heat in a furnace with a Fletcher burner, and pass a steady stream of perfectly dry chlorine gas over the heated mass. The chloride formed freely, and condensed in beautiful silky needles of a pure white colour, at a point just beyond the red-hot zone of the tube; the part next the tube fused to a white mass. In the earlier experiments the chloride was yellow, due to the presence of iron; but in all the experiments some patches of a light blue colour were noticed, though the reason of this was not ascertained. Ground lump-sugar is by far the best reducing agent to use, as on charring it makes a very intimate mixture with the oxide, is practically free from ash of any kind, and absolutely free from iron, a very minute quantity of which utterly ruins the appearance of the chloride, giving it a strong yellow colour. It is important that a steady stream of chlorine be maintained,

as a sudden rush carries over specks of carbon and unaltered oxide, thus contaminating the chloride; it is difficult absolutely to avoid this source of error.

The main difficulty was to rake out the chloride sufficiently expeditiously into perfectly dry, wide-mouthed bottles, as the anhydrous chloride is exceedingly hygroscopic; a little care is also necessary to avoid raking out any of the carbon from the tube. In a tube 2 feet long and $1\frac{3}{8}$ in. internal diameter, I was able to operate on forty grams of oxide at a time, though, as a rule, about ten grams of ignited oxide were taken. For operating on small quantities, a Hoffman furnace and hard combustion-tube were used.

Chlorides were prepared from a variety of samples of basic carbonate of glucinum that had been fractioned with ammonium sesquicarbonate, and from various samples of hydrate purified from the last traces of alumina by solution in hydrofluoric acid, and treatment with acid potassium fluoride. The actual yield of anhydrous chloride was taken by weighing the dry bottle before and after placing the charge in it, but a great part was often lost by absorption of moisture before it could be got out of the tube. In each case the carbonaceous residue was removed by the other end of the tube, and ignited to find how much oxide remained unaltered. The following are the particulars of a few of the results obtained:—

In one experiment 10 grams of glucinum oxide ignited with 20 grams of sugar gave 25 grams of anhydrous chloride, and 1 gram of unaltered residue.

In another experiment 10 grams of pure oxide of glucinum mixed with 40 grams of sugar, and charred; then chlorinated in the porcelain tube, at a bright red heat, in a gas furnace, with a good stream of well-dried chlorine, gave 15·5 grams of anhydrous chloride. The residue gave, on ignition, 1·6 grams of oxide left unacted on—time, three hours. The chloride was in pure white crystals, with a grey powder intermixed, undoubtedly due to a little fine carbon, and possibly some of the oxide being carried over by the current of chlorine. A good part was unfortunately left in the tube and lost, or rather washed into the residue bottle. Of course, any chloride left in the tube was not allowed to mix with the carbonaceous residue before ignition to determine the proportion of unaltered oxide.

In a third experiment 13·5 grams of pure oxide, ignited with four times its weight of sugar, and, chlorinated as above, gave 23·75 grams of anhydrous chloride; and the residue, on ignition, gave 3·6 grams of unacted-on oxide. The chloride in the centre of the tube was in beautiful tufts of needle-like crystals; and round the tube were found crystalline crusts of the anhydrous chloride.

Other experiments gave similar results. Theoretically 10 grams of anhydrous oxide of glucinum should yield about 32 grams of anhydrous chloride.

A very careful analysis was made of the chloride from the last experiment. The principal difficulty in the analysis of this substance was to weigh accurately

the portion taken, as the anhydrous chloride of glucinum absorbs moisture with extreme rapidity. A quantity of the crystals were quickly dropped into a perfectly dry weighing-tube, stoppered, and the whole accurately weighed; then dissolved in the weighing-bottle, and transferred to a beaker, filtered from any specks of carbon, and made up to one litre. The weight taken was 5·6782 grams, and, deducting the weight of the carbon and oxide, ·0040 gram, this gives 5·6742 grams of chloride per litre. The ammonia precipitate from two portions of 100 cc. each gave ·1814 gram and ·1773 gram of ignited oxide, or an average of ·1793 gram GlO per ·5674 gram of chloride taken. The silver nitrate precipitate from two similar portions gave 2·0114 grams and 2·0130 grams of silver chloride, an average of 2·0122 grams containing ·4975 gram of chlorine, and equivalent to ·1123 gram of oxygen, taking the atomic weights of oxygen as 16, chlorine as 35·45, and silver as 107·93. Now, ·1793 GlO less ·1123 oxygen = ·0670 Gl, and ·0670 Gl plus ·4975 chlorine = ·5645 $GlCl_2$.

The quantity actually taken was ·5674, the difference being due to moisture absorbed during transference of the chloride from the tube to the bottle, and from thence to the weighing-tube; but this may be neglected, as the analysis shows that the ratio of glucinum and chlorine actually present in the compound is as ·0670 to ·4975.

Calculating the analysis from the glucinum and chlorine actually found, this gives—

	Expt. Per cent.	Theory. Per cent.
Glucinum, ...	11·87	11·38
Chlorine, ...	88·13	88·62

The theoretical results are calculated on the accepted atomic weight of glucinum, 9·1. If the equivalent is calculated from the experimental results, we get—

·4975 : 35·45 : : ·0670 : eq. Gl = 4·77

Taking the now almost universally accepted view that glucinum is a diad, we have for the atomic weight 9·54, a figure considerably higher than that determined from the ignition of the sulphate.

In a number of other analyses of chlorides prepared from glucinum, extracted and purified in different ways, variations were observed in the ratio of glucinum and chlorine that could not very well be explained on the supposition of the presence of aluminium chloride. I intend to investigate this subject farther.

Anhydrous chloride of glucinum is freely soluble in absolute alcohol, with great rise of temperature, and the solution conducts electricity.

When a little of the chloride is put in a test-tube, and the tube immersed in the vapour of boiling sulphur, the chloride melts easily, but does not boil.

Evidently glucinum chloride melts at about 400° C., and boils at about 500° C.; and I cannot understand how Carnelly gave the melting-point as between 585° C. and 617° C., and maintained the accuracy of these figures after Nilson and Pettersson pointed out that it boiled at 520° C. There is absolutely no doubt that anhydrous glucinum chloride melts below, and near to, the boiling-point of sulphur, 440° C., and that its vapour condenses just below a red heat, as can easily be seen when its preparation is conducted in a glass tube. As a red heat begins somewhere between 500 and 550°, according to the manner in which the observation is made, this agrees very well with Nilson and Pettersson's figures.

X. Metallic Glucinum.

To reduce the anhydrous chloride to the metallic state, a piece of sodium was pressed into the bottom of a good-sized nickel crucible, the anhydrous chloride shot in, more sodium placed on the top, and the crucible gently heated until the reaction went off. The lid, which was blown off, was quickly replaced with a pair of tongs, and the whole crucible plunged into absolute alcohol to dissolve out the excess of sodium, boiled with fresh portions of absolute alcohol once or twice, and then finally washed with hot water. There always seemed to be a good deal of action when the water was first applied; and this was, no doubt, due to the impossibility of completely removing all the sodium hydrate; and caustic soda has a vigorous action on glucinum. After washing, the water had no further action on the glucinum, even on boiling. The glucinum thus obtained was in the form of a dark grey powder, which assumed a bright grey or white metallic lustre under the burnisher.

A nickel crucible is quite unacted on by metallic sodium, and comparatively large quantities may be operated on with safety, if no water is near into which the molten sodium might find its way in the event of an accident. The lid is usually shot off when the reaction starts, and the whole charge catches fire; but this is at once extinguished by replacing the lid with a pair of tongs. It is well to have a thick glass plate in front of the crucible to protect the eyes when the action starts.

An attempt was made to fuse the metal, but without success. When heated by the electric arc, in an atmosphere of hydrogen, the metal volatilized without fusion, and condensed to a grey metallic mirror, like arsenic.

Royal College of Science for Ireland,

17*th May*, 1904.

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By RICHARD J. MOSS, F.I.C., F.C.S.

[Read, June 21; Published, November 3, 1904.]

Although no compound of helium has been artificially produced, it is possible that the element may exist in a chemically combined state in the minerals from which it is obtained. The methods employed to liberate helium from these minerals are such as may, and in many cases certainly do, give rise to chemical decompositions. On the other hand, the fact that helium is derived from the emanation from radium affords a strong argument in favour of the supposition that the gas exists in some chemically uncombined form in the radio-active minerals.

With the view of throwing some light upon this point, I carried out the following experiments on a specimen of pitchblende said to be from Joachimsthal, but having a specific gravity of only 5·32. It is part of a specimen which has been in the Science and Art Museum, Dublin, for about forty years.

The pitchblende was broken into fragments of about 2 to 3 grammes each, and placed in a desiccator over sulphuric acid for several days.

The fragments, weighing 72·7 grammes, were quickly transferred to the hopper of a mill for grinding substances *in vacuo*, and which will form the subject of a future communication. A bell-glass which covers the mill, having been warmed in the water oven, was placed in position, and the Töpler pump with which the apparatus was connected was immediately started, and kept in action until the limit of exhaustion had been reached. When it was found that working the pump for ten minutes failed to remove any trace of gas, the apparatus was allowed to stand. The next day, on working the pump, a minute bubble of gas was obtained; it was too small to suggest leakage, and was discarded. The grinding of the mineral was now started, and immediately I noticed that the tube containing phosphorus pentoxide, which formed part of the connection between the grinding-mill and the pump, became warm, showing that water was liberated as a result of the pulverisation of the mineral. At the same time the pump, which was kept going continuously, began to bring over some bubbles of gas. The whole operation of grinding the mineral lasted seven minutes.

Having completely exhausted the apparatus, the gas was removed, and found to measure 0·092 cubic centimetre at 0° C. and 760 mm. pressure. The phosphorus pentoxide tube was found to have gained in weight 0·0199 gramme. This weight of water corresponds to about 24 c.c. of water vapour at 0° C. and 760 mm. pressure; water is, therefore, the chief substance liberated in the pulverisation of the pitchblende.

The gas was examined by exposing it to the action of potassium hydroxide, and then heating a fragment of dry phosphorus in it. The results obtained were as follows, the volumes being calculated for 0° C. and 760 mm. pressure:—

Original volume of gas,	... ...	0·092
After action of KHO,	... ...	0·078
,, ,, of P,	... ...	0·076

From these results the composition of gas, assuming only carbon dioxide and oxygen to be removed by the alkali and phosphorus, is—

		Per Cent.
CO_2, ...	0·014	15·41
O, ...	0·002	2·33
Other gases, ...	0·076	82·26
	0·092	100·00

The quantity of gas available was too small for detailed examination; accordingly I introduced it into a thoroughly exhausted Plücker tube with magnesium terminals. The volume to which the gas was expanded in this operation was approximately 22 cubic centimetres; its pressure would, therefore, be 2·6 mm. On passing the current from a small induction coil, giving a spark of about 7 centimetres, I obtained a discharge which gave only the nitrogen spectrum. On introducing a jar and spark-gap, a deposit of magnesium was rapidly formed on the sides of the Plücker tube, and the nitrogen spectrum faded rapidly, and in a few seconds was succeeded by an almost pure helium spectrum.

In this experiment the pitchblende had been ground to a rather coarse powder; there were many fragments fully a millimetre in diameter. This powder was again placed in the mill, and the operations above described were repeated. The volume of gas now obtained was 0·0941 c.c. This gas was treated as in the former experiment, but the attempt to determine the quantity of oxygen was not successful; a very slight increase in volume instead of a diminution was caused by the action of hot phosphorus. The gas was, however, introduced into the Plücker tube with magnesium terminals. The spectra of nitrogen and helium were both visible, but almost immediately the nitrogen spectrum disappeared.

The connections attached to the syphon tube, by which the gas had been admitted, were so arranged that by lowering a mercury reservoir a certain amount of gas would flow from the Plücker tube to the other parts of the apparatus, and on raising the reservoir the opposite effect was produced. By circulating the gas in this way while the discharge was passing in the Plücker tube, all the nitrogen was quickly absorbed. The tap communicating with the Sprengel pump was now opened, and the gas pumped off and measured. Its volume was found to be 0·0454 c.c., and I conclude from the appearance of its spectrum that it was practically pure helium. The results of the various measurements corrected for temperature and pressure were as follows:—

Original volume of gas, ...	0·094
After action of KHO, ...	0·077
After removal from the Plücker tube, ...	0·045

The composition of the gas is accordingly:—

		Per Cent.
CO_2, ...	0·017	18·35
He, ...	0·045	48·27
N, &c., ...	0·032	33·38
	0·094	100·00

In the second grinding, which it will be observed yielded almost exactly the same volume of gas as the first grinding, the quantity of water obtained was 0·1020 gramme. This is about five times as much as was obtained in the first case. I believe this marked difference is due, in part at any rate, to absorption of moisture by the powder from the air. The first determination of water was probably correct; but the conditions under which the second determination was made did not admit of the necessary accuracy.

The helium obtained in the first grinding was unfortunately lost; but, considering the similarity in composition of the two lots of gas, so far as they were examined, it is perhaps admissible to conclude that the quantity of helium in the first lot was about the same as in the second lot. This would give a total of 0·09 c.c. of helium, or 0·00125 c.c. of helium per gramme of pitchblende, while the carbon dioxide liberated in the two grindings corresponds to 0·0004 c.c. per gramme of the mineral.

I found that the second grinding had reduced about half the pitchblende to a powder, 62 per cent. of which passed through a sieve having meshes measuring about 0·17 mm. between the wires (100 meshes to the inch), 36 per cent. passed through a 0·4 mm. mesh, and the remaining 2 per cent. passed through a 0·8 mm. mesh.

I next proceeded to ascertain the quantity of helium and carbon dioxide in this powder by decomposing it chemically. For this purpose 25 grammes of the powder was mixed with 65 grammes of hydrogen potassium sulphate. The mixture was placed in a capacious glass tube, which was attached by a mercury-jacketed rubber joint to a combustion tube 50 cm. in length and 1·5 cm. diameter, containing granulated copper oxide which had been heated to redness, and cooled in an atmosphere of oxygen. This tube was attached by another mercury-jacketed rubber joint to a tube 25 cm. long, and 7 mm. in diameter, packed with phosphorus pentoxide, and leading to the Töpler pump. The copper oxide was heated to low redness, and the tubes were thoroughly exhausted. The next day, having ascertained that no leakage had taken place, the copper oxide was heated to redness, and the hydrogen potassium sulphate was fused, and kept in that state for three hours. The pump was kept working during the whole operation, and while the fused mixture was cooling. The gases evolved were collected in four lots. These were free from sulphur dioxide, except the last lot, which contained some of that gas which was removed by moist manganese dioxide. The total volume of gas thus obtained was 73·797 c.c. (reduced to 0° C. and 760 mm. pressure).

On absorbing carbon dioxide, the volume was reduced to 4·911 c.c.; alkaline pyrogalol caused a further contraction to 4·799 c.c. This gas was now mixed with an equal volume of pure oxygen, and sparked in an eudiometer; there was no alteration in volume, as might be expected, nor was carbon dioxide produced. The volume was now made up to about 26 c.c. by the further addition of pure oxygen, and the mixture was sparked in the usual way over a solution of caustic alkali for three hours, when contraction seemed to have ceased. To ensure as complete a removal of nitrogen as possible, the sparking was continued for two hours more. On removing the excess of oxygen by combustion with phosphorus, the residual gas was found to measure 1·596 c.c. This was introduced into a Plücker tube with magnesium terminals, and with a phosphorus pentoxide tube attached. These tubes had been previously exhausted until the Plücker tube showed green phosphorescence, and the current passed with difficulty. Notwithstanding the long-continued sparking with oxygen over alkali, the spectrum at first observed was that of nitrogen, with the green line of mercury prominent. On introducing a Leyden jar and spark-gap into the secondary circuit, and passing the current for a few minutes, the nitrogen was absorbed, and the spectrum now obtained showed only the helium and mercury lines. By circulating the gas through the Plücker, tube as already described, while the current was passing, the nitrogen was removed from the gas in all parts of the apparatus. The gas, now consisting of helium alone, was pumped off and found to measure 1·560 c.c.

The composition of the gas obtained by the decomposition of this pitchblende, neglecting sulphur dioxide and hydrogen, removed by the action of the hot copper oxide, is therefore

		In 100 Parts.
CO_2, ...	68·886	93·35
O, ...	·112	·15
He, ...	1·560	2·11
N, ...	3·239	4·39
	73·797	100·00

An examination of the fused mass remaining from the decomposition of the pitchblende showed that the coarser parts of the powder had not been decomposed. The mass was treated with water, and the undecomposed pitchblende was separated by elutriation and found to weigh 10·3 grammes; this deducted from the 25 grammes originally taken, leaves 14·7 grammes decomposed. The undecomposed portion had probably lost part of its helium; but assuming this loss to be negligible, the yield of helium was 0·106 c.c. per gramme of the pitchblende, and the yield of carbon dioxide was 4·686 c.c. per gramme.

The total quantity of helium obtained both by grinding and by chemical decomposition was 0·107 c.c. per gramme, and of this total the yield by grinding alone constitutes 1·17 per cent. In the case of carbon dioxide the total yield by both methods was 4·686 c.c. per gramme, and only 0·0085 per cent. of this was obtained by grinding. From a specimen of calcite from the limestone in the neighbourhood of Dublin, I obtained by grinding *in vacuo* 0·024 c.c. of carbon dioxide, from each gramme of the mineral, along with sulphuretted hydrogen, hydrocarbons, nitrogen, and water. In this case there can be no question as to the gas being enclosed in cavities. Under a magnification of 200 diameters numerous cavities, some with liquid enclosures, can be detected. The heat resulting from the mechanical action of grinding, though it is inappreciable, might liberate some combined water; but the liberation of carbon dioxide and other gases can scarcely be attributed to any thermal effect. Now, the volume of combined carbon dioxide in 1 gramme of calcite is 222·4 c.c.; the enclosed carbon dioxide, liberated by grinding, is, therefore, 0·011 per cent. of the combined gas. In the specimen of pitchblende the corresponding proportion is 0·0085—a ratio of approximately the same order. There is accordingly a very strong presumption, though no cavities can be detected in the pitchblende by microscopic examination, that they nevertheless exist, and contain carbon dioxide, other gases, and water. It is difficult to resist the conclusion that the helium, which is liberated by grinding in much larger proportion relatively to the total quantity present, is also contained in

cavities. It is well known that many minerals contain vast numbers of minute cavities. Zirkel estimates that there are as many as 360,000,000 cavities in a cubic millimeter of Hiaünite from Melfi. It is evident that if such minute cavities contain gas, even at high pressure, the quantity of gas that would be liberated by grinding the mineral to a fine powder would necessarily be small; the most minute sub-division that can be effected by mechanical means would have the effect of opening a comparatively small proportion of the cavities.

Assuming that the helium is contained in pitchblende in such cavities, the question arises whether any notable quantity of the gas escapes from the mineral at ordinary temperatures. To throw some light upon this point, I enclosed 14 grammes of the powdered mineral in a tube, along with some fragments of caustic potash, separated from the pitchblende by glass wool. In a prolongation of the tube there was some phosphorus pentoxide, and attached to this prolongation was a branch leading on one side to a Plücker tube with magnesium terminals, and on the other side to a Sprengel pump. The tube was now exhausted until it showed the usual green phosphorescence. The Plücker tube containing the pitchblende was jacketed with water which was slowly brought to the boiling point. The discharge was now of a bluish-violet colour. Its spectrum showed the hydrogen lines 6562 and 4341, but the most prominent lines were those of mercury. I may observe that a few globules of mercury had been accidentally mixed with the pitchblende. The tubes were now exhausted until the discharge passed with difficulty. Next day the mercury lines were again prominent in the spectrum, but there was also a faint banded spectrum of nitrogen. On continuing the current, the mercury and nitrogen spectrum faded, and the yellow, green, and blue lines of helium became faintly visible; but in a short time the resistance of the tube increased, and the green phosphorescence made its appearance. I continued to observe the spectrum of the discharge from time to time for nineteen days. Every day the helium lines became more prominent, and finally no other spectrum except that of mercury accompanied the helium spectrum. On the twentieth day I pumped off the helium until the tube ceased to conduct. A small bubble, measuring 0·022 c.c., was obtained. Each gramme of the mineral had yielded in nineteen days 0·00016 c.c. This is less than one-tenth of the quantity obtained in a few minutes by pulverizing the mineral *in vacuo*. It is evident that if any gas diffuses from the mineral at the ordinary temperature and pressure, the quantity must be very small.

Another specimen purchased as pitchblende from Joachimsthal was broken into fragments of about 2 grammes each, and picked as free as possible from matrix ; its specific gravity at 15°·5 C. was 6·83. The fragments, weighing 225 grammes, were placed in a desiccator over sulphuric acid, and kept there for twelve days. At the

expiration of that time the mineral was rapidly transferred to the grinding apparatus, which was then closed and exhausted. The mineral was now reduced to powder, an operation which occupied thirty minutes. The pump was kept going during the grinding, and for an hour afterwards. In this way a quantity of gas, measuring 1·155 c.c. at the standard temperature and pressure, was collected. An analysis of this gas gave the following results:—

		Per Cent.
CO_2, ...	0·429	37·60
O, ...	0·049	4·29
H, ...	0·049	4·29
N, ...	0·433	37·95
He, ...	0·181	15·87
	1·141	100·00

It will be observed that the total of the gases determined is 0·014 c.c. less than the original volume. This I cannot account for, unless it is due to incomplete combustion of hydrogen. When the carbon dioxide, oxygen, and hydrogen had been removed, the excess of oxygen employed for the endiometric determination of hydrogen was removed by combustion with phosphorus, and the residual gas was found to measure 0·614 c.c. instead of 0·628, the difference between the sum of the gases determined and the original volume. This may be accounted for on the supposition that the combustion of hydrogen in the endiometer was incomplete.

On the removal of the gases, as above described, the apparatus was allowed to stand over-night. The pumping was then resumed, and at occasional intervals for six hours the apparatus was exhausted to the utmost limits with the Töpler pump. Gas came over in gradually diminishing quantity, and no doubt a further minute quantity of gas might have been collected had the operation been continued for some hours longer.

The gas obtained in this second pumping measured 1·186 c.c., and gave the following results on analysis:—

		Per Cent.
CO_2, ...	0·608	51·53
O, ...	·024	2·03
H, ...	·008	·68
N, ...	·537	45·51
He, ...	·003	·25
	1·180	100·00

On removing the tube of phosphorus pentoxide through which the gases from the grinding apparatus had passed to the pump, it was found to have gained 0·100 grammes in weight, so that in this case, as in previous experiments, by far the most abundant substance liberated by grinding the mineral *in vacuo* is water vapour.

The total quantity of the helium obtained in the above experiment is 0·181 + 0·003 = 0·184 c.c., or in the proportion of ·00092 c.c. for each gramme of the mineral.

The total quantity of helium in the mineral was determined as before by fusing with potassium hydrogen sulphate. Each gramme of the mineral gave 0·082 c.c. The helium liberated by grinding *in vacuo* is therefore 1·11 per cent. of the total quantity present—a very close approximation to the 1·17 per cent. obtained with the other specimen of pitchblende.

The occurrence of hydrogen in this case is noteworthy. When the gas, mixed with oxygen, was sparked in the eudiometer, there was an unmistakable explosion, and a contraction which was appreciable before the gas was transferred to the measuring apparatus. There was no change in volume when the gas was exposed to the action of moist caustic potash. There can be little doubt, therefore, that the gas was hydrogen. I did not detect hydrogen in the other specimen of pitchblende. A small quantity might have escaped detection. In a radium-bearing mineral the presence of hydrogen in cavities containing water might be anticipated.

I hope to submit a further communication on other minerals in which helium has been found.

The Laboratory, Royal Dublin Society,
Leinster House, Dublin.

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[February 20, 1905.]

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BY

W. E. ADENEY, D.Sc.,

Curator and Examiner in Chemistry in the Royal University of Ireland.

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[Read, December 20, 1904; Published, February 20, 1905.]

The question of the possibility of atmospheric gases being transmitted through water by any process other than that of diffusion, or by thermal currents, or mixing, does not appear to have been considered.

Hüfner* found, in his work on the diffusion coefficients of gases in water, that, if a gas be placed above the liquid, the phenomenon of diffusion is disturbed by downward streaming effects through the water. He explains these as being caused by the water becoming heavier as it dissolves the gas, and sinking to the bottom.

Hüfner, in order to avoid this streaming effect, employed a thin plate of the porous mineral hydrophane to hold up a column of water in an experimental tube, and so provided the means of introducing the gas at the bottom of the water, and of studying the upward diffusion of the gas.

The author has met with this downward streaming in the course of some experiments recently made upon the solution of atmospheric gases by water. It did not, however, appear to him that the cause of the streaming, as suggested by Hüfner, offered a wholly satisfactory explanation of the phenomenon observed. He accordingly thought that a careful investigation of this streaming effect might be followed by results of some value.

Method of Experiment.

Glass tubes, about two metres long and 18 mm. bore, were employed for all the experiments to be described, except for a few preliminary ones. The dimensions of these will be given with the descriptions of the experiments.

The tubes were nearly filled either with distilled water or with sea-water, and warmed and exhausted, as far as desirable, by means of a mercury-pump. The corks closing the lower ends of the tubes were fitted with fine glass capillary tubes, to provide the means of attachment to the mercury-pump for exhaustion, and for drawing off samples.

* Ann. Phys. Chem., 1897 (11), vol. lx., pp. 134–168.

After the gases in solution and in the air-spaces had been abstracted, the corks closing the upper ends of the tubes were removed; and the tubes for most of the experiments were immersed to a depth of about 1300 mm. in a large water-jacket, which was kept cool by a constant flow through it of water from the main supply.

The object of the water-jacket was to prevent the formation of downward convection currents, by keeping the lower portions of the water-columns in the tubes at a slightly lower temperature than the upper portions, which were exposed to the temperature of the laboratory.

In most of the experiments to be described, the surfaces of the water-columns were kept constantly agitated by means of currents of air drawn through their upper layers. For the few preliminary experiments to be described, the tubes were fitted with corks carrying fine glass inlet and exit tubes of 3 mm. bore, and for the later experiments with glass capillary tubes of 0·5 mm. bore. The inlet tubes dipped at different depths, from 10 to 200 mm., below the surface.

Slow currents of air were employed, at the rate of about 1000 cc. per hour, with the smaller bore inlet tubes, and at somewhat quicker rates with the tubes of the larger bore.

In a few cases the surfaces of the water-columns were agitated by mechanically-rotated glass stirrers, just dipping below the surface of the water. In these cases, the experimental tube employed was of wider bore to allow of stirrers of a gridiron shape being used.

When the surfaces of the water-columns were left unbroken, the experimental tubes were loosely covered to protect their contents from dust.

The height of the columns of water was in nearly all experiments about 1800 mm. In all experiments the surface of the water was freely exposed to the air. Observations of barometric pressures were not thought necessary.

At the conclusion of each experiment, layers of the water of 100 to 200 mm. thickness were drawn from different depths from the surface, and stored in tubes standing over mercury until they could be analysed.

The dissolved gases in these samples were extracted by boiling with a little sulphuric acid in vacuo. The apparatus employed for this purpose, and for the analysis of the gases, has already been described by the author in his memoir on dissolved gases and fermentative changes.*

Of the results of the analysis of the dissolved gases, only those for the nitrogen are recorded in this communication. These are expressed in cc. at 0° C. and 760 mm. bar.

* Trans. Royal Dublin Society, 1895, vol. v., Part xi.; "The Course and Nature of Fermentative Changes in Natural and Polluted Waters, and in artificial Solutions, as indicated by the Composition of the dissolved Gases."

DESCRIPTION AND RESULTS OF EXPERIMENTS.

Experiments with unbroken Surfaces.

No. 1. *Two tubes, each* 1950 *mm. long and* 18 *mm. bore, were filled to the height of* 1800 *mm. from the bottom, one with sea-water, and one with distilled water, both being nearly nitrogen-free, and were immersed in the water-jacket, and kept undisturbed for twenty-eight days. The temperature of the room varied between* 16·5° *and* 10° *C., that of the jacket between* 12·9° *and* 8·0° *C. During the last week of the experiment the temperature of the room varied between* 12° *and* 10° *C., and that of the jacket between* 9° *and* 8° *C.*

Distilled water saturated at 10° *C. contains* 15·37 *cc.* N_2 *per litre. Sea-water saturated at* 10° *C. contains* 12·47 *cc.* N_2 *per litre.*

Depth of layer below surface.	Sea-water.	Distilled water.
1 to 200 mm.	9·24	11·50
300 to 500 mm.	8·96	11·10
800 to 1000 mm.	8·91	8·43
1600 to 1800 mm.	7·89	5·84
Before aëration	1·51	1·55

No. 2.—*Similar to No.* 1, *with the exception that the tubes were kept longer in the water-jacket, viz., for* $2\frac{1}{2}$ *months. Temp. of room at close of experiment,* 17·8°; *of the water-jacket,* 15·6° *C. Distilled water at* 17·8° *C. contains* 13·29 *cc.* N_2 *per litre; sea-water at* 17·8° *C. contains* 10·80 *cc.* N_2 *per litre.*

1 to 200 mm. ...	10·80 ...	12·56
1600 to 1800 mm. ...	10·72 ...	9·09

The foregoing results reveal a number of points of interest in connexion with the streaming effect above referred to.

The most noticeable is the marked difference in rate in sea-water and distilled water, being much greater in the former than in the latter. Another point of interest is, that the surface layers of the sea-water and distilled water in No. 1, even after exposure to the air for twenty-eight days, did not become saturated with nitrogen. Hence we may conclude that the streaming effect is more rapid in both sea-water and distilled water than the rate of solution at the surface exposed to the air.

These experiments, so far as they go, may be regarded as supporting the suggestion of Hüfner, that the streaming is a gravitational effect due to concentration currents. The following experiment, however, affords conclusive evidence that the streaming is really a gravitational effect:—

No. 3.—*A* U*-shaped tube, each limb* 1600 *mm. long and* 50 *mm. diameter, was filled*

with boiling sea-water, and immediately closed air-tight with indiarubber corks. When the water had cooled, the tube was further exhausted by means of the mercury-pump. One limb was then uncorked, and the water in it was saturated with atmospheric gases by a current of air drawn through the surface-layer to a depth of 50 *mm. for forty-eight hours. After this, a sample was drawn from the bottom of the open limb, and one from the lower portion of the closed limb, about* 200 *mm. from the bottom. The gases were extracted from these, and the nitrogen in each determined with the following results :—*

Open limb.	Closed limb.
12·12	1·49

Six months later another sample was collected from the lower portion of the closed limb, as before, and the nitrogen in it determined. It was 1·73.

Experiments with broken surfaces.

The experiment No. 1, and several others which were made during the earlier part of this investigation, but which it is unnecessary here to record, since the results were similar, shows that a retardation of solution takes place at the surface of the water exposed to the air. Experiments were accordingly made in which the surface of the water exposed to the air was kept continuously broken.

No. 4.—*A tube,* 1100 *mm. long and* 50 *mm. diameter, was filled with nitrogen-free sea-water, and immersed in the water-jacket to a depth of* 1025 *mm. A glass stirrer of a gridiron-shape,* 40 *mm. broad, was fixed to dip* 25 *mm. below the surface of the water, and was mechanically rotated for nineteen hours. At the end of this time the temperature of the upper layer of the water-column above the water-jacket was* 8·3°, *and that of the water-jacket* 7·8° *C. Samples were drawn from the top and bottom layers of the water-column, and the nitrogen determined in each with the following results :—*

Top layer.	Bottom layer.
12·69	12·45
At commencement,	1·51

No. 5.—*A similar experiment was made with distilled water with results as follows :—*

13·29,	11.02
At commencement,	1·46

Sea-water saturated at 8·3° C. contains 12·91 cc. N_2 per litre. Distilled water saturated at 8·3° C. contains 15·92 cc. N_2 per litre.

In order to provide in these experiments the means of detecting whether or not the mechanical agitation of the surface-layers of the water-columns caused a circulation of the water to the bottom, a large crystal of potassium bichromate was placed at the bottom of the tubes; but in no case was the dissolved bichromate

observed to rise above the immediate neighbourhood of the crystal, and become mixed with the water above it.

The results of the last two experiments proved, as was anticipated, that the downward streaming takes place so rapidly, when the surface of the water exposed to the air is continuously agitated, that long columns of water are completely saturated with atmospheric gases with comparative rapidity.

Experiments were next made in which the surfaces of the water-column were kept broken by means of slow currents of air. It was thought that by their means it would be easy to make comparative observations on the effect of agitating different thicknesses of the surface-layers of two or more columns of water by the same disturbing cause.

No. 6.—*Two tubes, each* 1980 *mm. long and* 24 *mm. bore, were filled with nitrogen-free sea-water. Each tube was fitted with an indiarubber cork, and fine glass inlet and exit tubes of* 3 *mm. bore, and was connected in series, so that the same current of air could be drawn through the surface-layers of each column of water. The inlet tube, through which the current of air was first drawn, dipped* 100 *mm. below the surface, and the second inlet tube dipped* 10 *mm. below the surface of the second column of water. The current of air was not previously filtered. It was continued for twenty-five hours. At conclusion, temperature of room was* 19·5°, *and of the jacket* 15·7° *C.*

Sea-water saturated at 19·50° C contains 10·50 cc. N_2 per litre.

Depth of layer below surface.	1	2
1 to 100 mm., ...	10·57	8·01
1500 to 1600 mm., ...	8·91	4·08
Before aëration, ...	1·58	1·56

No. 7.—*Similar to the experiment No.* 6, *with the exception that the inlet tube dipped* 10 *mm. below the surface in the first column of water, and* 100 *mm. in the second column. The current of air was continued for twenty hours. Temperature of room at conclusion was* 18·2°, *and of the jacket* 14·9° *C.*

Sea-water at 18·2° C. contains 10·68 cc. N_2 per litre.

Depth of layer below surface.	1	2
1 to 100 mm., ...	10·07	9·95
1000 to 1100 mm., ...		6·05
1200 to 1300 mm., ...	9·61	
1700 to 1800 mm., ...	6·26	2·15
Before aëration, ...	1·61	1·56

The sea-water employed for the experiments thus far recorded was collected in Dublin Bay. It had been stored in partially full bottles for some weeks previous to use. It contained 19·57 grams chlorine per litre.

From the foregoing results it appears that the rate of aëration, as a consequence of the streaming, was not materially affected by difference of depth of the air

inlet tubes, when two water-columns were aërated in series by the same current of air. The inlet tubes dipped 100 mm. and 10 mm. below the surfaces in the columns of water 1 and 2, respectively, in experiment No. 6, and *vice versa* in experiment No. 7; and yet both the columns of water which first received the air-currents in the two experiments were found to have dissolved more nitrogen than either of the second columns of water.

It appears therefore from these experiments that something was taken from the air by the first water-column in each experiment in addition to the ordinary atmospheric gases, and that very little reached either of the second columns.

The next experiments were arranged with a view of ascertaining whether, by using slower currents of air and fine capillary glass inlet and exit tubes of 0·5 mm. bore, the streaming effect could be completely confined to the first water-column.

No. 8.—*Three tubes, each containing sea-water nearly nitrogen-free, were fitted with indiarubber corks and air inlet and exit tubes, and were connected in series, so that the same current of air could be drawn through the upper layer of each column of water to a depth of* 200 *mm., one after the other.*

The air-current was a slow one, at the rate of about a litre per hour. It was continued for forty-eight hours, and then stopped. Samples of water were then drawn from each tube as before, and stored in glass vessels standing over mercury. The nitrogen determinations are given below. The tube which the air-current first entered was marked 1, *and the one it next entered* 2, *and so on. The air was filtered through glass wool.*

Temperature of room, . . 12·8° to 10·0° C.
Temperature of jacket, . . 8·6° to 8·3° C.

Sea-water at 10° C. contains 12·47 cc. N_2 per litre.

Depth of layer.	1	2	3
1 to 200 mm., ..	12·24	11·15	11·09
800 to 1000 mm., ..	8·20	2·29	2·55
1600 to 1800 mm., ..	8·13	2·18	2·54
Before aëration, ..	1·57	2·13	2·51

No. 9.—*Similar to the experiment No.* 8, *with the exception that the air inlet tubes dipped only* 50 *mm. below the surface. The air-current was filtered through glass wool.*

Temperature of room, . . 13·7° to 13·5° C.
Temperature of jacket, . . 8·4° to 8·5° C.

Sea-water at 13·5° C. contains 11·67 cc. N_2 per litre.

Depth of layer.	1	2	3
1 to 200 mm., ..	10·81	7·15	6·93
300 to 500 mm., ..	10·60	1·61	1·52
800 to 1000 mm., ..	9·36	1·65	1·56
1600 to 1800 mm., ..	9·20	1·65	1·52
Before aëration, ..	1·75	1·63	1·51

These results show that, when the current of air is sufficiently washed during its passage through the surface-layer of the first column of sea-water, it may be passed through the surface-layers of other columns of sea-water, and no streaming effect occurs, although the layer of water, through which the air is bubbled, soon becomes saturated with atmospheric nitrogen.

It may therefore be concluded that the streaming effect is not due to concentrated solution currents, as suggested by Hüfner, but to something in the air besides its chief gaseous constituents, and that when that something is washed out, the subsequent solution of the gases is not attended by streaming.

The sea-water employed for these experiments was also collected in Dublin Bay. It was filtered through glass wool, and stored in glass bottles for a week before use. It contained 19·45 grams chlorine per litre.

Similar experiments were made with distilled water, as there were reasons for anticipating that the cause of the streaming would not be so rapidly washed out by distilled water as by sea-water.

Experiments with Distilled Water.

No. 10.—*Similar to experiment No.* 8, *with the exception that distilled water was used instead of sea-water.*

Temperature of room, . . 11·5° to 11·8° C.
Temperature of jacket, . . 8·5° to 9·0° C.

Distilled water at 11·8° C. contains 14·83 cc. N_2 per litre.

Depth of layer below surface.	1	2	3
1 to 200 mm.,	14·60	12·27	12·20
300 to 500 mm.,	11·62	11·10	11·18
800 to 1000 mm.,	4·18	4·20	4·48
1600 to 1800 mm.,	3·54	3·05	3·20
Before aëration,	1·55	1·73	1·97

No. 11.—*Similar to No.* 10, *with the exception that the air-current was not filtered.*

Temperature of room, . . 12° to 10° C.
Temperature of jacket, . . 8·7° to 8·5° C.

Distilled water saturated at 10° C. contains 15·37 cc. N_2 per litre.

Depth of layer below surface.	1	2	3
1 to 200 mm.,	13·81	12·77	12·64
300 to 500 mm.,	12·20	11·17	11·08
800 to 1000 mm.,	5·20	5·17	5·14
1600 to 1800 mm.,	4·15	4·06	4·00
Before aëration,	1·75	1·55	1·51

These experiments confirm the conclusion which was drawn from the results of experiment No. 1, viz., that the downward aëration of distilled water by the streaming is not so rapid as the aëration of sea-water by the same means.

They also show that the cause of the streaming was not concentrated in the

first water-column, as was the case with sea-water, but that, on the contrary, it affected nearly equally the three water-columns. It may be inferred from this last observation that the streaming being less in distilled water than in sea-water, is due to the something causing it to be more slowly taken out of the air-current by the former than by the latter.

The following experiment was also made with distilled water, wide bore, 3 mm., inlet and exit tubes being employed. The inlet tube dipped 25 mm. below the surface of the water:—

No. 12.—*A tube* 1980 *mm. long and* 24 *mm. bore was filled with nitrogen-free distilled water, and the air-current continued for twelve hours.*

Temperature of room, . .	15·5° C.
Temperature of jacket, . .	12·9° C.
Depth of Layer.	
0 to 200 mm., ...	10·56
650 to 800 mm., ...	3·9
1500 to 1600 mm., ...	2·68
Before aëration, ...	1·51

The question arises, What is the active cause of the streaming? The author does not desire at this stage of his investigation to advance definitely any explanation of the facts observed. One, however, which is suggested by the experiments that have been described, may be pointed out. It is that the streaming is possibly caused by minute dust-particles, or by other centres of condensation possibly of an electrical nature, carried by the air-current and taken up by the water, together with the gaseous constituents of the air; and that these bring about in some way a sufficient density of the dissolved or liquefied gases to render it possible for them to be drawn gravitationally downwards through the water.

The fact that the streaming is more marked in sea- than in distilled water may be explained by assuming that the sea-water, from the salts which it holds in solution, is able to make more use of the centres of condensation introduced by the air which is bubbled through it. When sea-water and distilled water are separately shaken up with the air, the difference of behaviour of the two waters is, as is well known, very marked, it being easy to obtain a formation of small bubbles of air in sea-water to an extent quite impossible in distilled water.

There can be no question from the experiments recorded that, if the air be completely washed, the surface of sea-water exposed to it will become saturated with nitrogen, but no streaming effects will take place.

It is equally evident from these experiments that, whatever may subsequently be discovered to be the true cause of the streaming, its effect in large volumes of sea- or river-water under natural conditions must be of great importance, and of such dimensions that the effect of ordinary diffusion may, in comparison, be entirely neglected.

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ON SECONDARY RADIATION.

BY

J. A. McCLELLAND, M.A.,
Professor of Experimental Physics, University College, Dublin.

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VOLUME VIII.—(SERIES II.)

XIV.

ON SECONDARY RADIATION.

BY

J. A. McCLELLAND, M.A.,

Professor of Experimental Physics, University College, Dublin.

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XIV.

ON SECONDARY RADIATION.

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[Read, December 20, 1904; Published, March 3, 1905.]

The following pages give an account of an investigation of the secondary radiation given off by bodies when they are exposed to a strong primary radiation from radium.*

In the present paper the secondary radiation produced by the more penetrating radium rays only—the β and γ rays—has been considered; the nature of this secondary radiation and its relative intensity in the case of different substances have been studied. The subject of secondary radiation seems of some importance and promise, as there are many problems on which it may have a possible bearing; it is closely related to the subject of spontaneous radio-activity and to all phenomena produced by the discharge from bodies of particles charged with electricity.

Secondary radiation under the action of cathode rays has been investigated by Starke,† Austin and Starke,‡ Swinton,§ and others. Secondary radiation under the action of Röntgen rays has been studied by Perrin,‖ Townsend,¶ and others; while secondary radiation due to radium rays has been detected and investigated to some extent by Becquerel** and others.

Apparatus.

The secondary radiation was detected and measured by its ionising power. Fig. 1 gives a sketch of the apparatus used. T is a brass tube, 20 cms. in length

* When the work described in this paper was completed and the paper partly written, one on the same subject by Mr. Eve appeared in the Philosophical Magazine, December, 1904; the points discussed in the two papers are not, however, by any means always the same. In cases where the same points are discussed, the agreement is on the whole good, although in some cases somewhat different deductions have been made.

† Ann. d. Physik u. Chemie (Wied.), lxvi., 1898, p. 49. Ann. d. Physik (Drude's), Vierte Folge iii., 1900, p. 75.

‡ Ann. d. Physik (Drude's), Vierte Folge, ix., 1902, p. 271.

§ Proc. Roy. Soc., lxiv., 1899, p. 377.

‖ Ann. de Chimie et de Physique, tom. xi., 1897, p. 496.

¶ Proc. Camb. Phil. Soc., vol. x., 1899, p. 117.

** Comptes Rendus, tom. cxxxii., 1901, pp. 371, 734, 1286.

and 3·2 cms. internal diameter, connected to small storage-cells, by which it could be kept at any required potential: a metal rod is fixed along the axis of the tube, being insulated by paraffin and joined to a Dolezalek electrometer, the joining wire being suitably protected. The radium is placed at R so as to send a pencil of rays through a hole in a thick lead screen, other lead screens being placed to protect the tube T from direct radiation.

A plate of the substance under examination is placed at P, and the secondary radiation from it enters the tube T, and produces ionisation, which is measured in the usual way by the electrometer. The end of the tube T is covered with a

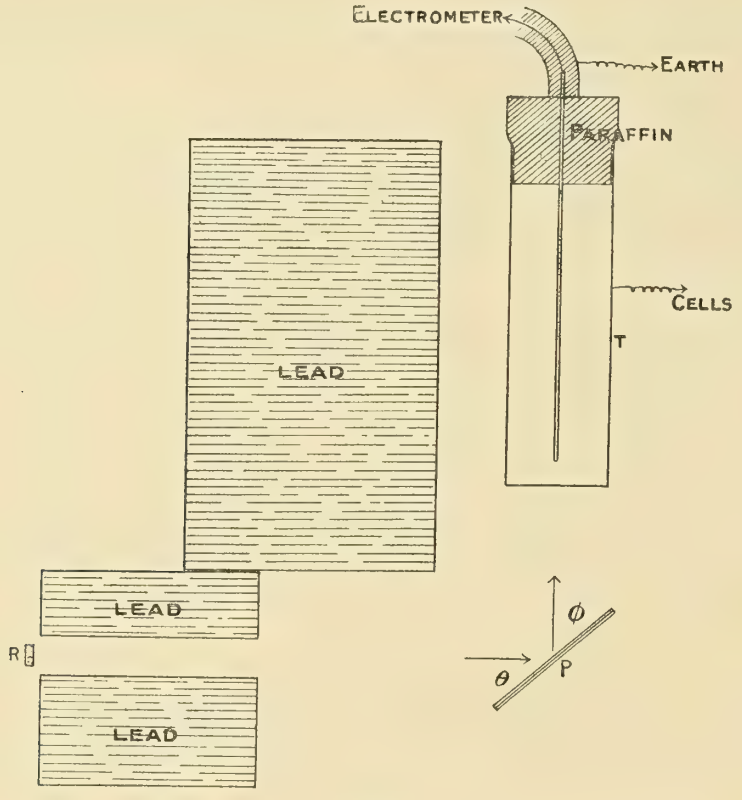

Fig. 1.

single sheet of tinfoil. The distance from the radium to the plate P was in most of the experiments about 26 cms., and from P to the nearest point of T was usually about 9 cms. The distances are given in every case where they are of importance.

In every experiment the ionisation observed in T is corrected for whatever small conductivity existed between the tube T and its inner terminal when the plate P is not in position, this small conductivity being due partly to the normal ionisation inside T, and partly to insufficient screening of direct radiation from

the radium, and also to secondary radiation from the air traversed by the primary rays.

Fifty milligrams of radium were used, enclosed in a vessel which stopped the α radiation.

Effect of the Position of the Plate.

With the apparatus described, it is easy to detect a secondary radiation which travels in all directions from the part of the plate struck by the primary rays. The first point investigated was whether the amount of secondary radiation depended on the angle of incidence of the primary rays and on the angle that the testing-tube T made with the plate; whether, in fact, there was anything of the nature of 'reflexion' of the primary rays. This is a point which has been investigated by several observers using cathode rays as the primary rays; and while some observers have found a well-marked maximum of secondary radiation in the direction obeying the law of reflexion, others have not obtained such a result.

The point was first tested by keeping the direction of the primary rays and that of the tube T constant and at right angles, as in the figure, while the lead plate P was tilted; and this experiment showed a well-marked maximum when θ was 45°.

The following observations were made, the secondary radiation being expressed in an arbitrary scale:—

	Secondary Radiation.
$\theta = 22\frac{1}{2}°$, ...	73
$\theta = 45°$, ...	100
$\theta = 67\frac{1}{2}°$, ...	71

The observed numbers are reduced so as to express the maximum by 100.

The plate P was then set so that ϕ was 45°, and this angle was kept constant, the direction of the primary rays only being changed. We then get as follows:—

	Secondary Radiation.
$\theta = 25°$, ...	93
$\theta = 45°$, ...	100
$\theta = 65°$, ...	97
$\theta = 80°$, ...	92

In the third case the primary rays and the plate are both tilted, keeping the angle between them constant and equal to 45°, while the angle ϕ changes. We have then:—

	Secondary Radiation.
$\phi = 20°$, ...	84
$\phi = 45°$, ...	100
$\phi = 70°$, ...	98

The first experiment shows a decided maximum when $\theta = 45°$; the other two experiments can be interpreted as showing a maximum when the law of reflexion is fulfilled with the secondary radiation falling off unequally on the two sides of the maximum position.

The numbers indicate that, superposed on an effect of the nature of reflexion, we have other effects; the law of reflexion being fulfilled, we get more secondary radiation the more we approach the normal, which is to be expected, as then the radiation coming from some depth in the plate has a smaller thickness to penetrate.

The general effect is, therefore, a radiation in all directions from the part of the plate struck by the primary rays with a decided maximum in one direction, due probably to a sort of reflexion of the impinging particles. Many points described later show that the whole effect is not due to a simple scattering of the primary rays, but that it consists of a true secondary radiation from the plate.

It may also be stated here that experiments with a magnetic field, to be described later, show that the secondary rays we are now dealing with consist of negatively charged particles. These travel through several centimetres of air, and also penetrate the tinfoil covering the end of the ionisation tube T before they produce the effect by which they are detected; there may be also a more easily absorbed radiation from the plate which has not been studied in the present paper.

Secondary Radiation not merely a Surface Effect.

The secondary rays are not produced merely at the surface of the plate struck by the primary rays; it is easy to show that they come from all parts of a layer of considerable depth, the depth depending on the substance of which the plate is composed.

This point was tested by using, for the plate in fig. 1, a single sheet of tinfoil (about ·013 mm. thick) in the first place, then two sheets, and so on. The secondary radiation was as follows:—

Sheets of Foil.	Secondary Radiation.
1	13
2	24
4	44·5
8	60·5
16	73·5
32	77

Fig. 2 is plotted from these numbers.

We should, of course, expect the secondary rays to come from all depths reached by the primary, unless the penetrating power of the secondary be less than that of the primary, in which case the effective thickness of plate will be determined by the penetrating power of the secondary.

The penetrating power of the secondary rays will be discussed later, but it

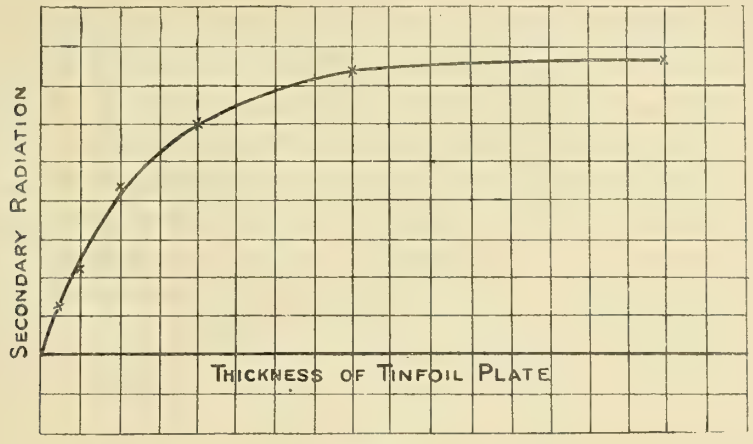

Fig. 2.

may be pointed out here that the above numbers indicate that the secondary rays must at least approximate in penetrating power to the primary. The second sheet of tinfoil contributes almost as much secondary radiation as the first, and the rays from the second sheet must pass through the first before reaching the testing-tube T; the secondary rays must therefore suffer no very great loss of intensity in passing through the sheet of foil.

Analysing the Primary Radiation (β and γ rays) with respect to its power of producing Secondary Rays.

The primary rays used in the above experiments were a pencil of β and γ rays. It was necessary to investigate what part of the secondary rays was due to the more penetrating part of the primary, and what was due to the less penetrating primary rays.

This was done by making two separate experiments. In the first the apparatus was used as in fig. 1, and successive layers of tinfoil were placed in front of the lead screen, so that the primary rays passed through them, and the intensity of the secondary rays was measured at each step.

In the second experiment the radium with the thick lead screen was placed as in fig. 4, so that a pencil of primary rays fell directly on the testing-tube T, travelling parallel to its axis; and, as before, sheets of tinfoil were placed in front

of the radium, and the ionisation measured at each stage. We thus get from one experiment the intensity of the primary rays after they have passed through various thicknesses of foil; and from the other experiment we get a measure of the secondary rays produced by the primary after passing through the same thickness of foil as before.

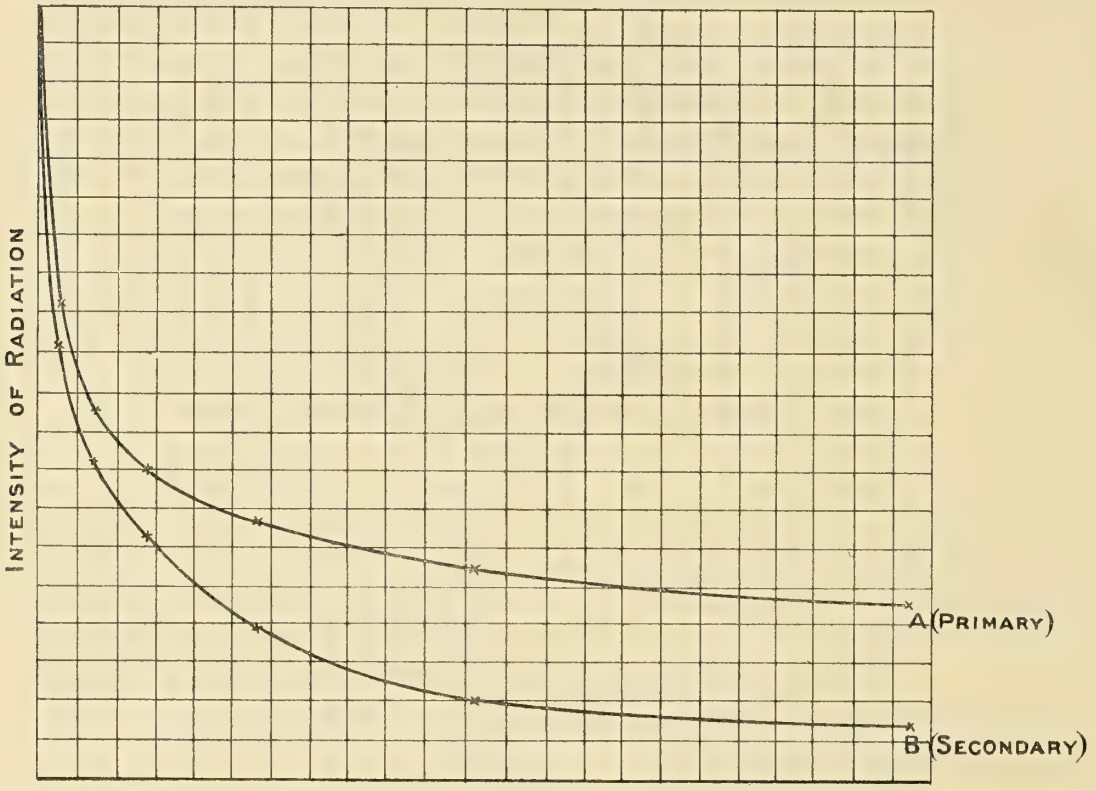

Fig. 3.

The results are as follows, the primary and secondary radiation being expressed so as to have a maximum of 100 in each case:—

Sheets of Foil.	Primary Radiation.	Secondary Radiation.
0	100	100
1	61	56
2	47·5	41
4	40	31
8	33	19
16	27·5	10
32	23	7

The curves in fig. 3 are plotted from the above numbers. The curves show, at a glance, the relative efficiency of the various parts of the primary pencil in pro-

ducing secondary radiation. The less penetrating β rays are more efficient than the more penetrating; and the efficiency falls off more and more as we get into that part of the curve A corresponding to the γ rays. But even when the curve A has become practically horizontal—that is, when there is nothing left in the primary pencil but γ rays—there is still some secondary radiation.

The magnetic experiments described later show that this part of the secondary radiation—the part produced by γ rays—also consists of negatively charged particles.

Different Substances as Sources of Secondary Rays.

The experiments described in this paper have all been carried out (unless when otherwise stated) with a lead plate as the source of secondary rays.

A large number of substances have been tested as sources of secondary rays. The substances were placed in the form of plates at the position P, fig. 1. The experiment described above, in which tinfoil was used, shows that it is important when comparing substances to use in each case a thickness sufficient to give the maximum effect; this was done when comparing different substances.

The following list shows the results obtained, the radiation from lead being taken at 100 :—

Substance.	Secondary Radiation.	Density.	Atomic Weight.	Secondary Radiation. / Atomic Weight.
Lead,	100	11·4	206·4	0·48
Platinum,	92	21·5	194·3	0·47
Tin,	80	7·3	118·8	0·67
Brass,	75	8·4		
Zinc,	65	7·2	65·0	1.00
Copper,	64	8·9	63·2	1·01
Iron,	62	7·8	56·0	1·10
Glass,	31	2·5		
Aluminium,	33	2·7	27·0	1·22
Ebonite,	27	1·15		
Cardboard,	23	0·80		
Wood,	19	0·52		
Paraffin,	18	0·90		

The numbers show that, taken generally, the substances of greater density produce the greater secondary radiation, but there are many exceptions to the rule,

and the secondary radiation is by no means proportional to the density; platinum, for example, produces less radiation than lead, and tin more than copper.

It gives a more interesting result to compare the secondary radiation with the atomic weight of the substance; the atomic weight and the ratio of the secondary radiation to the atomic weight are given in the table. It will be seen that throughout the table, the greater the atomic weight the greater is the secondary radiation. The ratio is not constant, but it will be noticed that the substances fall into groups with respect to this ratio. There is a well-marked group for which the ratio is unity, and another for which the ratio is one-half. Of course the absolute value of the ratio is merely a question of the scale in which the secondary radiation is expressed.

It is intended to extend largely the list of substances examined to see if the dependence of the radiation on the atomic weight holds throughout, and to follow out the apparent grouping.

The radio-active substances—radium, thorium, and uranium—are all substances of high atomic weight; and the results now before us indicate that the greater the atomic weight the greater is the secondary radiation under the action of β rays. The secondary radiation seems therefore to be closely allied to the spontaneous radiation from radio-active substances. The impact of the β rays produces a disturbance sufficient to cause the disintegration of the atom in substances which, in the normal state, are in stable equilibrium.

The dependence of the secondary radiation on the atomic weight, and the close relation between it and radio-activity, suggest that the activity of radium might be increased by the action of its own radiations, and Voller (Physikalische Zeitschrift, Dec. 1, 1904) has obtained indications of such an action. We must remember, however, that the secondary radiation we are considering is one of β particles; so that, in extending our table to radium, we should consider a transformation product of radium which emits β particles.

It is interesting to compare the table given above with the results of observers who have studied the small radio-activity of ordinary substances when there is no artificially applied exciting cause. McLennan found that lead was about twice as active as zinc, while tin gave an intermediate value; this is the same order as in our table of substances. A comparison with numbers given by Strutt for the activity of a number of substances does not, however, show much agreement.

In the above comparison of different substances, the secondary rays passed through a single sheet of tinfoil ·013 mm. thick; another set of observations was taken with the end of the tube T covered with a sheet of aluminium ·3 mm thick, in addition to the tinfoil. No difference in the order of the substances took place; but if we again express the secondary radiation from lead as 100, the

corresponding number for each substance is less than before, when the rays passed through tinfoil only; the relative diminution is different for different substances, and is particularly great for ebonite, cardboard, and paraffin. In other words, of all the substances examined, the secondary rays from lead are the most penetrating; those from paraffin, cardboard, and ebonite the least penetrating. The difference in penetrating power of the rays from the different substances is not very marked.

A rather rough experiment was tried to see if the temperature of the substance had any effect on its power of giving off this secondary radiation. A plate of copper was used, and its temperature increased by a strong bunsen-flame playing on the back of the plate: no change in the amount of secondary radiation was noticed. It should also be stated that no particular care was taken with the preparation of the surface of the different plates used; the surface was always well cleaned, but the degree of polish was not attended to, and did not seem to be of any importance.

As all numbers in this paper are given in arbitrary units, it may be useful to state a result specifically to give an idea of the absolute magnitude of the radiation we are dealing with: 50 milligrams of radium bromide are placed 26 cms. from a lead plate as in fig. 1, the rays from the radium passing through a hole 1·2 cm. in diameter in a lead screen; the lead plate is 9 cms. from the ionisation tube T; the electrometer gives a deflexion of 2000 scale-divisions per volt, and the total capacity of the system charged is ·001 microfarads; the secondary radiation then produces a deflexion of 115 scale-divisions per minute.

Relative Importance of Secondary β Rays. Penetrating Power of Secondary β Rays.

In making some experiments on the penetrating power of the secondary β rays, and comparing it with that of the primary rays, some results were obtained

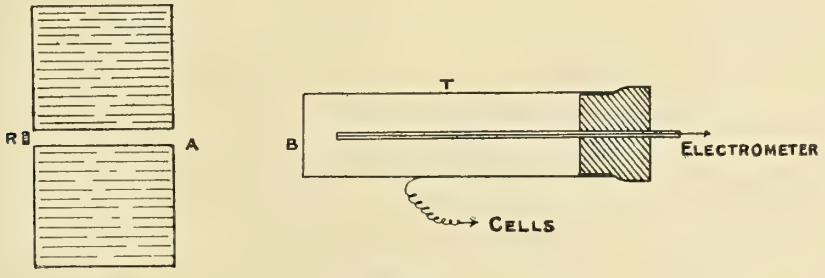

Fig. 4.

which show that very different values may be deduced for the absorption of β rays according to the apparatus used, and which also show very forcibly the importance

of secondary effects when dealing with this type of radiation. Suppose we place the radium so that a pencil of β (and γ) rays falls directly on the ionisation tube T as in fig. 4. If, now, we proceed to measure the absorption produced by, say, a screen of cardboard, the result we get depends very much on where we place the cardboard in the path of the rays.

In an experiment, the distance AB, fig. 4, was 26 cms., the internal diameter of the tube T being 3·2 cms.

The result was as follows :—

	Ionisation in T.
No screen, ...	100
Screen at B, . . .	71
Screen 5·5 cms. from B, . . .	62
Screen 10·5 cms. from B, . . .	54
Screen 26·0 cms. from B, . . .	36

The screen produces a different effect in different positions, because it is a source of radiation which travels in all directions from the part struck by the primary rays; and therefore the nearer the screen is to the testing-vessel T, the greater is the observed ionisation. If the secondary rays were of much less penetrating power than the primary, the observed ionisation would, of course, be increased by moving the screen towards the vessel T; but we have seen there is no very great difference in penetrating power between the primary and secondary rays, so that the chief cause of the effect noted must be the fact that the screen becomes a source of radiation spreading out in all directions. Of course we have also tertiary radiation from the air and from the end of the testing-tube.

The numbers given above show that, after passing through the sheet of cardboard, the secondary rays are of as great importance as the primary rays; for the ionisation with the screen close to the testing-vessel is twice what it is with the screen 26 cms. away.

Another example of the importance of the secondary rays is given by results obtained in comparing the penetrating power of the secondary rays with that of the primary pencil consisting chiefly of β rays. The radium was first placed as in fig. 4, and the primary rays examined by placing different thicknesses of tinfoil, first at B and then at A. The apparatus was then arranged as in fig. 1; and the absorption of the secondary rays measured by placing sheets of tinfoil at the end of the tube T.

The results are as follows:—

Sheets of Foil. Each ·013 mm. thick.	Primary Rays.		Secondary Rays.
	Tinfoil 26 cms. from Ionisation Tube *T*.	Tinfoil at Ionisation Tube *T*.	Tinfoil at Ionisation Tube *T*.
No foil	100	100	100
1	60	96	—
2	47	90	80
4	39	77	64
8	32	66	44
16	27	49	26
32	22	35	9

Curve *A*, fig. 5, refers to the primary rays when the absorbing tinfoil is placed 26 cms. from the ionisation tube; curve *A'* to the primary rays, with the foil

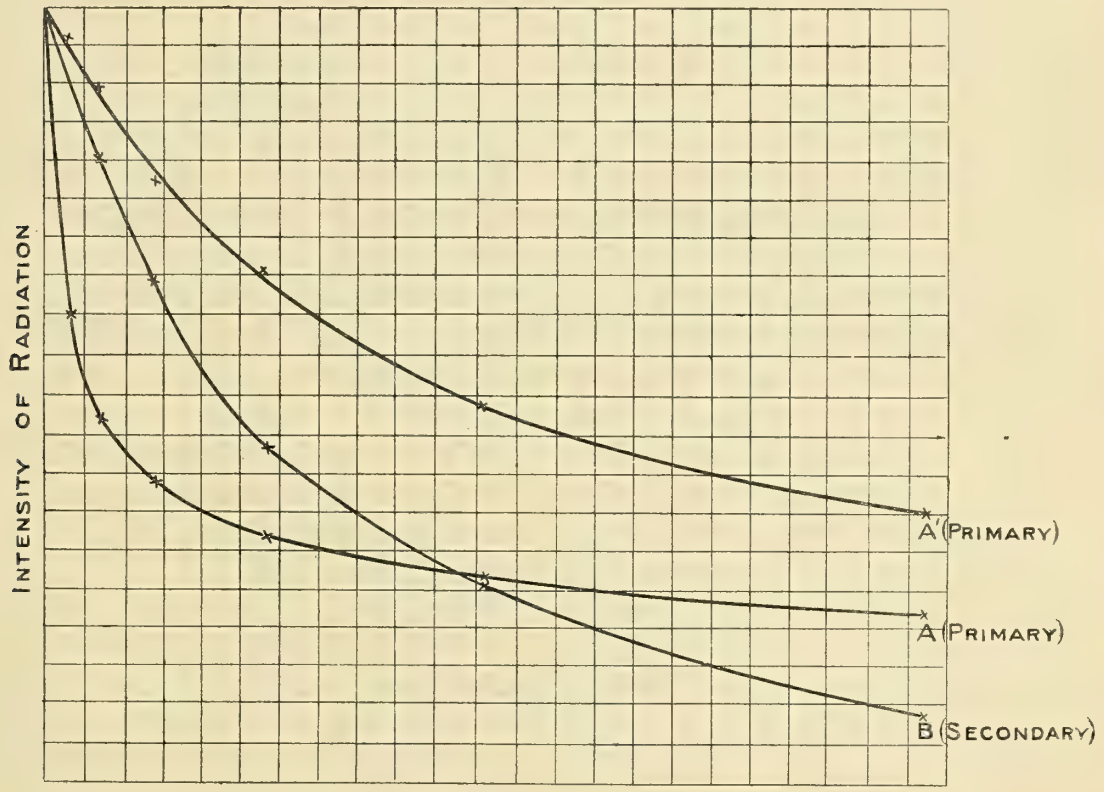

Fig. 5.

placed close to the ionisation tube; curve *B* to the secondary rays. Curves *A*

and A' show the importance of the part played by the secondary rays, and how they may influence the value calculated for the coefficient of absorption. Comparing curves A' and B, in both of which the foil is placed close to the ionisation tube, the secondary rays appear to be more easily absorbed than the primary; but the conditions of the experiments are not quite the same in the two cases, as the secondary rays are not confined to a narrow pencil as in the case of the primary, and therefore the relative position of the absorbing tinfoil is not so important. The difference in penetrating power is probably, therefore, not as great as a comparison of the curves A' and B would suggest.

It will be observed that the primary curves A and A' become practically horizontal, which is, of course, due to the presence in the primary pencil of the very penetrating γ rays. The secondary curve B, on the other hand, continues to descend with increasing thicknesses of foil; this is because the secondary pencil consists entirely of β rays. The magnetic experiments showed that it was possible to deflect all the secondary rays. The experiments therefore did not detect any γ rays produced by the stoppage of the primary β particles, or by the starting of the secondary β particles; such rays may have existed in small amounts and escaped detection, as we must remember that, in every case, a correction has to be made for the small normal ionisation in the tube T.

The importance of the secondary rays in the above experiments suggests a more detailed consideration of the manner in which matter is penetrated by a stream of charged particles like β rays; whether, in fact, there is great penetration by the primary particles, or whether it is not a successive stopping of one set and starting of another set of particles. The secondary rays, from the above experiments, are not very different in penetrating power from the rays that produce them, and the magnetic experiments described below indicate that the secondary rays contain some even more penetrating than the primary. The point is under further consideration.

Magnetic Deflexion of the Secondary Rays.

The nature of the secondary rays has been examined by passing them through a strong magnetic field. The apparatus, fig. 1, was modified by interposing between the plate P and the ionisation vessel T the poles of an electromagnet, so that the rays had to pass for a distance of 7·7 cms. through a horizontal magnetic field; the total distance from plate P to vessel T was 15 cms. If the secondary rays consist of charged particles, they should be deflected up or down, and thrown off the end of the tube T. It was found that, as the magnetic field was gradually increased, the ionisation in T gradually diminished, reaching a minimum which was not reduced by a further increase of the field. When this stage was reached, if the poles were allowed to come close together, thus interposing in the path of the rays a thickness of 7·7 cms. of iron, no further reduction of the ionisation took place, showing that this residual ionisation was not due to radiation from P, but simply to the normal ionisation in T, perhaps increased by some direct radiation from the radium. This residual ionisation was, therefore, subtracted from that observed at each stage of increase of the magnetic field, and the remainder represented the effect due to the radiation from the lead plate P.

The result is shown in column 2 of the following table :—

Magnetic Field.	Secondary β Rays.	Primary β Rays.
No field.	100	100
160	73	81
310	52	39
460	23	12
600	16	4
960	7	0
1250	0	0
2300	0	0

It was easy to show, by placing a thick lead screen in the path of the rays, so as to cut off the upper or lower half of the pencil falling on the tube T, and then applying a magnetic field, first in one direction, and then in the opposite, that the direction of deflexion of the rays showed them to be negatively charged. The fact that the residual ionisation with strong magnetic fields was not diminished by interposing several cms. of iron showed that there was no non-deflectable radiation of the nature of γ rays, or at least that it was too small to be detected by the apparatus used.

The primary β rays were examined in the same way, so as to get a comparison of the velocities of the primary and secondary, the mass and the charge being no doubt the same in the two cases. The radium, with the accompanying lead screen, was placed so that the primary rays fell directly on the tube *T*, passing between the poles of the magnet on the way. In this case a correction had to be applied for the γ rays which could not be deflected, and for the normal ionisation in *T*.

The result is given in column 3 of the above table; the maximum, when no magnetic field is applied, is represented by 100 for both primary and secondary rays. The numbers show that, while the first small field removes more secondary than primary rays, it requires a stronger field to remove completely the secondary. The secondary pencil seems to include some rays which are travelling with a greater velocity than any of the primary rays.

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XV.

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(Plate XI.)

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XV.

ON THE TEMPERATURE OF CERTAIN STARS.

By W. E. WILSON, D.Sc., F.R.S.

(Plate XI.)

[Read, January 17; Published, March 24, 1905.]

Numerous attempts have been made to classify the stars on a scale of temperature by a study of their spectra. Sir Norman Lockyer has proposed a rising and falling scale in which the highest temperature is represented by stars like Sirius, which give a brilliant, continuous spectra, extending well into the ultra-violet, and only crossed by a few dark lines, principally of hydrogen. Next in the order of lower temperature come stars like the Sun and Arcturus, whose spectra are darkened by numerous fine metallic lines; and then, lower still, are a class of reddish stars called IV-type, and also a very similar class called Wolf-Rayet, which generally give a spectrum in which the lines of titanium are very dark, as also the bands of carbon. It has been lately pointed out that these IV-type stars give a spectrum almost quite similar to that of a sun-spot. Lockyer has always maintained that a sun-spot is caused by the falling down of cool material on the Sun's photosphere, thus causing it to look dark. As these IV-type stars give a spectrum almost identically the same as a sun-spot, he naturally assumes that they also must be at a lower temperature than the Sun. The principal reason given for the coolness of a sun-spot is that in its spectrum some of the Fraunhofer's lines are both widened and darkened. I have lately taken a photograph of the spectrum of what I call an "artificial sun-spot"; and in this the dark lines are widened in exactly the same manner as in a real sun-spot, and this with absolutely no difference in the temperature of the absorbing gas, but merely by reducing the brilliancy of the source which gave the continuous spectrum (see Plate XI). The experiment was made in this way: an opal globe surrounding an electric arc represented the Sun's photosphere; on the globe was fixed a little piece of tissue-paper which stopped, say, 50 per cent. of the light; this was my sun-spot. Beyond this was a glass cell containing nitric oxide fumes; this represented the Sun's chromosphere, and gave me the dark-line spectrum. A lens was then arranged to form an image of the

paper patch on part of the slit of the spectroscope. We thus get in the photograph the band of continuous absorption due to the paper patch; and where the dark lines cross it, they are both widened and darkened. This, I think, clearly shows that there may be causes which account for the widening of the lines in sun-spot spectra other than the "cool downrush."

The Sun's photosphere is usually considered to consist of a layer of clouds formed of minute, solid particles of carbon,* or, possibly, calcium carbide. We know of no other element but carbon that could possibly remain solid at solar temperatures; and Prof. Hale quite lately has been able to observe the bright fluted spectrum of carbon by placing the slit of his spectroscope tangentially as close as possible to the solar image formed by the great Yerkes refractor.

Now, is it not conceivable that the temperature of the photospheric clouds is not very far from that at which carbon would be volatilized, and that a sun-spot is a local upheaval of the intensely hot gases which we know must exist below the photosphere where the clouds were removed? We should then only be receiving radiation from the gaseous layers which lie below the photospheric clouds; and, as these would be a much poorer source of radiation than the solid carbon particles, the spot would seem dark. Thus, I maintain that a sun-spot is a local region in which the temperature is so high that no substance can there remain in the solid form. Now, two arguments have been used against this supposition. First, it has been urged that if the temperature rose sufficiently to dissolve the clouds, they would simply re-form again at a greater altitude where the temperature was sufficiently low, and that therefore it would be quite impossible for a star to get rid of its photosphere by a rise of temperature. If this argument were pushed logically, then every element in the Sun should rise in the Sun's atmosphere until it got a temperature sufficiently low, and then form a cloud stratum.

That these different cloud-strata do not exist is, I think, sufficient evidence that carbon also, with a suitable rise of temperature, would be unable to form a cloud-layer. Secondly, it has been urged that, if a sun-spot be a hole in which we can see the deeper gaseous layers, the radiation coming from them—as they are thick enough to be opaque—ought to be quite as great as that coming from the photospheric clouds, and further, that the spot would not seem any darker than the photosphere.

The late Professor FitzGerald suggested, as a way out of this difficulty, that, in a layer of heated gases such as we have in the Sun, there must be powerful convection-currents, and that a quantity of the light coming from the lower layers would be reflected from the surfaces of these currents, and be returned back into the Sun. So that a layer of heated gas, no matter of what thickness, could never

* G. Johnstone Stoney, Roy. Soc. Proc., vol. xvii., 1868, pp. 1–57.

be as efficient a radiator as a layer of clouds. It is well to remember that a sun-spot is only dark by comparison, and is really quite as bright as the lime-light.

In the Sun, therefore, we seem to have two quite distinct sources of light which give us a continuous spectrum. First, we have the photospheric clouds, and secondly, at some depth below them, layers of highly heated gases, under considerable pressure, which are also able to radiate as if solid. Between these two layers lie the vapours of titanium, vanadium, and other elements which, from their atomic weights, lie at a greater depth in the solar atmosphere than carbon.

All the elements which, from their atomic weights, have a place near the photospheric clouds, must get carried up by convection-currents above them, and thus give us the Fraunhofer spectrum. In a sun-spot where the photospheric clouds are volatilized, we then get the absorption-spectrum of all the elements, like titanium, which lie between those two great sources of radiation, greatly strengthened.

Now, what are the causes which give rise to an absorption-spectrum? In the first place, the absorbing layer of gas must be at a lower temperature than the source which is giving the continuous spectrum; and, secondly, the absorbing layer must be thick enough to be opaque to those particular wave-lengths which give us the position of the dark lines. Gases are not all equally opaque. Sodium-vapour seems particularly opaque. The smallest amount of its vapour will show the *D* lines dark. Other gases, like hydrogen or helium, would probably require some miles of thickness before they would show their dark-line spectrum.

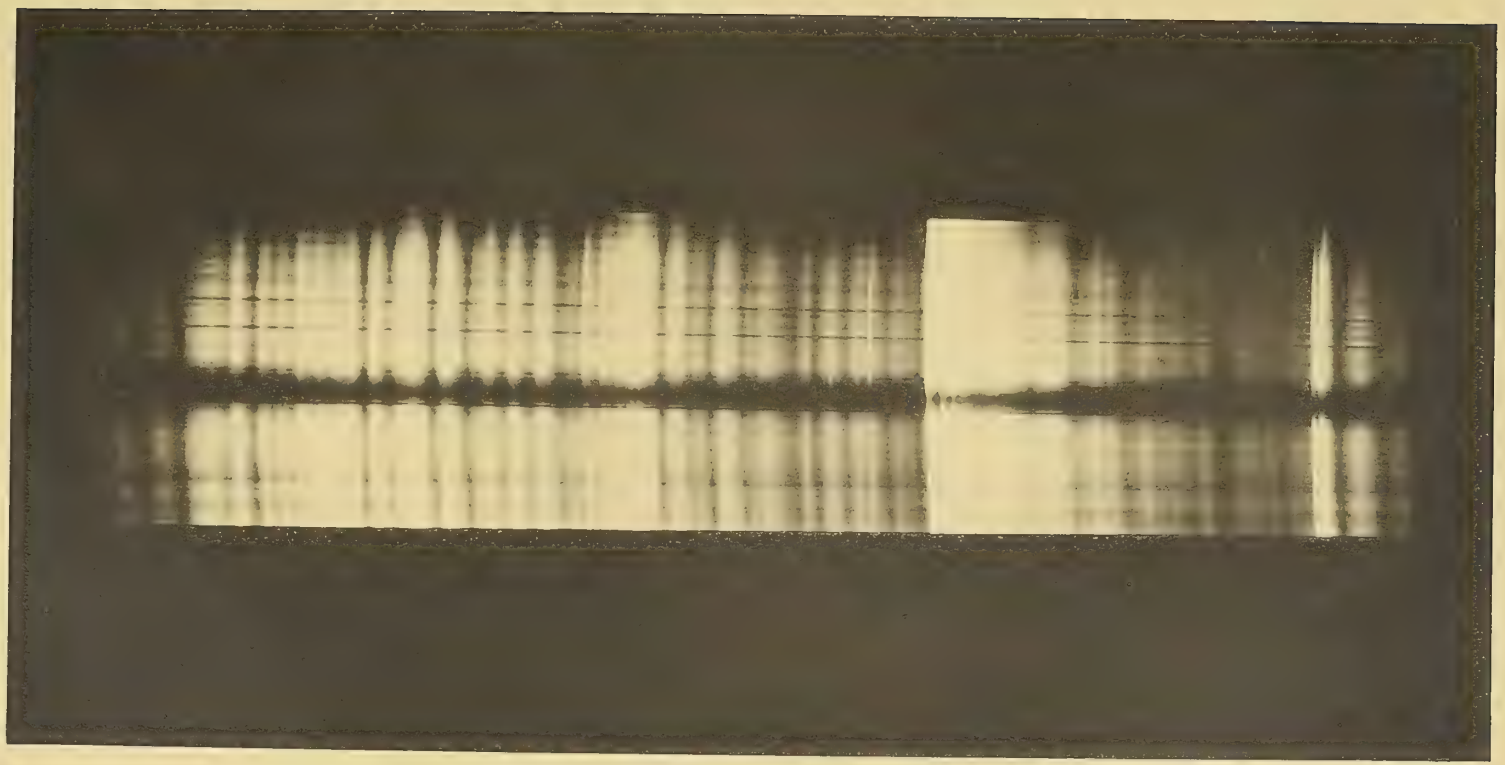
Gases are also not equally opaque for all their lines, which is shown by the gradual increase in the number of lines as the depth of the absorbing layer is increased. Therefore, in a sun-spot where the source of the continuous spectrum is at a greater depth, we see the lines in the absorbing layer increased in number, and both widened and darkened.

Now suppose the Sun's temperature were raised enough to volatilize entirely the photospheric clouds, his brilliancy would fall off probably fifty per cent., and his spectrum would be darkened by the increased lines of titanium and other elements which now lie below the photosphere. The bands of carbon would be dark; and probably the lines of magnesium and other elements which lie high up in his atmosphere would show as bright lines in his otherwise darkened spectrum. In fact, the solar spectrum would be almost identical with that of IV-type and Wolf-Rayet stars. Brilliancy, therefore, is, *per se*, no criterion as to the relative temperature of stars.

In Sirius, we have got a white star which Sir Norman Lockyer would class as at the highest point of the curve of temperature. Its spectrum is a brilliant, continuous one, extending far into the ultra-violet, and only crossed by a few dark lines, principally those of hydrogen. This type of spectrum would naturally be caused by such a deep layer of carbon clouds that they would be quite opaque to

any light coming from the gaseous layers below, and also so deep that the elements, which in the Sun are carried by convection-currents above the photosphere, and give us the Fraunhofer spectrum, would be unable to rise to that altitude. Thus, only elements like hydrogen, with an atomic weight less than carbon, would be able to draw their dark lines across his brilliant continuous spectrum. As the Sun seems to be so near the critical temperature at which the photospheric clouds are vaporized, it seems likely that Sirius is at a slightly lower temperature than the Sun, a temperature at which the star's photosphere is of great depth and stability.

It seems, therefore, most probable that IV-type stars are at a higher temperature than the Sun, and that Sirian stars owe their great brilliancy, not to very high temperature, but because their photosphere is a deep one, and its continuous spectrum is not darkened by the numerous lines, especially in the ultra-violet, which we have in the solar type of stars.



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XVI.

THE PARTIAL DIFFERENTIAL EQUATIONS OF MATHEMATICAL PHYSICS.

PART I.

BY

A. W. CONWAY, M.A., F.R.U.I.,

Professor of Mathematical Physics, University College, Dublin.

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[Read, December 20, 1904; Published, March 28, 1905.]

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I.—Introduction.

The linear partial differential equations of the second order, with constant coefficients, are of the greatest importance in Mathematical Physics; and their study has given rise to some of the most important functions of analysis. The full development appears to have been retarded for several reasons. The principal phenomena of physics to which they were applied were periodic; and hence attention was, for the most part, given to those solutions depending on circular functions. Again, there were no physical facts to lead to the consideration of singularities moving in space; and, lastly, there was the want of a general method of solution.

A method of great generality has been given by Whittaker in which singular solutions are built up from plane-wave solutions; but the reverse order appears more natural. In this paper the singular solution is placed first. By a general method, singular solutions of all classes of these equations are found sufficiently general to include all hitherto known singular solutions.

It is intended later to give the further consequences of these results as far as they are of interest in Mathematical Physics. Reference may be made

to Whittaker's *Modern Analysis*, chap. XIII.; T. H. Havelock, *Proceedings of the London Mathematical Society*, ser. 2, vol. ii., pt. II.; E. W. Hobson, *Encyclopædia Britannica*, 10th ed., 1902, vol. xxxii., p. 791.

II.—Classification of Equations and Fundamental Principles.

Considering, first, the case of homogeneous equations, the general form may be written

$$a_{11}\frac{\partial^2\phi}{\partial x_1^{\,2}} + 2a_{12}\frac{\partial^2\phi}{\partial x_1\partial x_2} + \ldots = 0,$$

the independent variables being $x_1, x_2, \ldots x_n$.

Let $A_{11}, A_{12} \ldots$ be the minors corresponding to $a_{11}, a_{12} \ldots$ of the determinant

$$\Delta = \begin{vmatrix} a_{11} & a_{12} & \ldots & a_{1n} \\ a_{21} & a_{22} & \ldots & a_{2n} \\ \ldots & \ldots & \ldots & \ldots \\ a_{n1} & a_{n2} & \ldots & a_{nn} \end{vmatrix},$$

and let S denote

$$A_{11}x_1^{\,2} + A_{12}x_1x_2 + \ldots;$$

then the following well-known results hold:—

$$a_{rs}A_{s1} + a_{r2}A_{s2} + \ldots + a_{rn}A_{sn} = 0,$$

or Δ, according as $r \neq$ or $= s$

$$a_{r1}\frac{\partial S}{\partial x_1} + a_{r2}\frac{\partial S}{\partial x_2} + \ldots a_{rn}\frac{\partial S}{\partial x_n} = 2x_r\Delta.$$

$$a_{11}A_{11} + 2a_{12}A_{12} + \ldots = n\Delta.$$

$$a_{11}\left(\frac{\partial S}{\partial x_1}\right)^2 + 2a_{12}\left(\frac{\partial S}{\partial x}\right)\left(\frac{\partial S}{\partial x_2}\right) + \ldots = 4S\Delta.$$

Substituting S^m for ϕ in the differential equation, we get

$$4m(m-1)\,\Delta S^{m-1} + 2mn\Delta S^{m-1}$$

which vanishes if

$$m = -\tfrac{1}{2}(n-2).$$

Hence $S^{-\frac{1}{2}(n-1)}$ is a solution of the differential equation. This is what we shall call the first fundamental singular solution.

If, however, $\Delta = 0$, then S becomes a perfect square, so that the fundamental solution becomes

$$(\lambda_1 x_1 + \lambda_2 x_2 + \ldots \lambda_n x_n)^{-(n-2)},$$

where

$$\lambda_1^2 = A_{11}\,;\ \lambda_1\lambda_2 = A_{12},\ \&c.\ \ldots\,;$$

but in this case any arbitrary function of

$$\lambda_1 x_1 + \lambda_2 x_2 + \ldots + \lambda_n x_n$$

will be a solution.

Introduce, now, new variables $\xi_1, \xi_2, \ldots, \xi_n$, such that

$$\xi_1 = \lambda_1 x_1 + \lambda_2 x_2 + \ldots + \lambda_n x_n$$

$$\xi_2 = \lambda'_1 x_1 + \lambda'_2 x_2 + \ldots + \lambda'_n x_n$$

$$\ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots$$

$$\xi_n = \lambda_1^{(n)} x_1 + \lambda_2^{(n)} x_2 + \ldots + \lambda_n^{(n)} x_n,$$

where the only restriction on the accented λ's is that the modulus of the substitution should not vanish. The differential equation will now be free from ξ_1, and will contain only $n-1$ variables. Proceeding as before, we can again form a singular solution. If its discriminant vanishes, we reduce the number of variables by one; and so, finally, we arrive at an equation of the form

$$\partial^2\phi/\partial\xi^2 = 0,$$

or else a singular solution whose discriminant does not vanish. This latter solution we shall for the most part study in what follows; and, unless otherwise stated, it will be supposed that the discriminant of the differential equation does not vanish.

If $x'_1 x_2 \ldots x'_n$ are simultaneous values of the variables (not all zero) satisfying

$$A_{11} x'^2_1 + 2A_{12} x'_1 x'_2 + \ldots = 0,$$

and if $x_1, x_2 \ldots$ are any other values such that

$$| x_r - x'_r | < \epsilon, \quad r = 1, \quad 2 \ldots n,$$

where ϵ is a small positive quantity, then

$$A_{11} x_1^2 + 2A_{12} x_1 x_2 + \ldots$$

is of the order ϵ, but if $x'_1, x'_2 \ldots x'_n$, are all zero, it will be of the order ϵ^2, *i.e.* the solution $S^{-\frac{1}{2}(n-2)}$ has an infinity of order $n-2$ at the origin, and an $(n-1)$-ply

infinite system of infinities lying on the hyper-surface $S = 0$. Now the type of solution which we shall require is one which will be infinite for a series of values of the variables which satisfy the equations

$$x_1 = f_1(u); \quad x_2 = f_2(u) \ . \ . \ . \ x_n = f_n(u),$$

which include, as special cases, the case of the functions

$$f_1(u), f_2(u) \ . \ . \ .,$$

being all constants, and the case of all but one being constant. Hence we shall classify the equations according to the different forms which the surface $S = 0$ may assume, or, what amounts to the same thing, the various forms assumed by the differential equation when expressed in canonical form.

By linear substitution, the equation may be written in one of the following three forms:—

$$\frac{\partial^2\phi}{\partial x_1^2} + \frac{\partial^2\phi}{\partial x_2^2} + \ . \ . \ . + \frac{\partial^2\phi}{\partial x_n^2} = 0, \qquad \text{(I)}$$

$$\frac{\partial^2\phi}{\partial x_1^2} + \frac{\partial^2\phi}{\partial x_2^2} + \ . \ . \ . + \frac{\partial^2\phi}{\partial x_n^2} - \frac{\partial^2\phi}{\partial x^2} = 0, \qquad \text{(II)}$$

$$\frac{\partial^2\phi}{\partial x_1^2} + \frac{\partial^2\phi}{\partial x_2^2} + \ . \ . \ . + \frac{\partial^2\phi}{\partial x_n^2} - \frac{\partial^2\phi}{\partial y_1^2} - \frac{\partial^2\phi}{\partial y_2^2} - \ . \ . \ . - \frac{\partial^2\phi}{\partial y_m^2} = 0, \qquad \text{(III)}$$

according as the equation in k

$$\begin{vmatrix} a_{11} - k & a_{12} & . \ . \ . & a_{1n} \\ a_{21} & a_{22} - k & . \ . \ . & a_{2n} \\ . \ . \ . & . \ . \ . & . \ . \ . & . \ . \ . \\ a_{n1} & a_{n2} & . \ . \ . & a_{nn} - k \end{vmatrix} = 0$$

has (I) all the roots of the same sign, (II) all the roots but one of the same sign, (III) not less than two roots of sign different from the others.

The second and less obvious type of singular solution is obtained as follows :—

If the number of variables be $n + 1$, the differential equation may be written

$$k\frac{\partial^2\phi}{\partial x^2} + a_{11}\frac{\partial^2\phi}{\partial x_1^2} + 2a_{12}\frac{\partial^2\phi}{\partial x_1 \partial x_2} + \ . \ . \ . = 0,$$

where there is no term containing the operator $\frac{\partial}{\partial x}$ to the first power. Then, with

the same meaning for the symbols as before, we can verify the following solution:—

$$S^{-\frac{1}{2}(n-2)}(kS + \Delta x^2)^{(\frac{1}{2}n-3)}(n > 3).$$

The third and general type of solution is as follows:—

Suppose the variables to consist of m groups $x'_1, x'_2, \ldots x'_{n'}$; $x''_1, x''_2, \ldots x''_{n''}$; $\ldots$; $x_1^{(m)}, x_2^{(m)} \ldots x^{(m)}{}_{n^{(m)}}$, and that the equation is so written that the products of operators out of different groups do not occur. If a'_{rs} be the coefficient of

$$\frac{\partial^2\phi}{\partial x'_r \partial x'_s}, \quad \&c.,$$

the following is a solution:—

$$S'^{-\frac{1}{2}(n'-2)} S''^{-\frac{1}{2}(n''-2)} S^{(m)-\frac{1}{2}(n^{(m)}-2)} \left(\frac{S'}{\Delta'} + \frac{S''}{\Delta''} + \ldots\right)^{\frac{1}{2}\Sigma n' - m - 2}.$$

III.—Equations of Class I.

The singular solution of

$$\frac{\partial^2\phi}{\partial x_1^2} + \frac{\partial^2\phi}{\partial x_2^2} + \ldots + \frac{\partial^2\phi}{\partial x_n^2} = 0$$

is, by what has been shown above,

$$(x_1^2 + x_2^2 + \ldots + x_n^2)^{-\frac{1}{2}(n-2)};$$

this is infinite only at the one point—the origin, and is there infinite to degree $n - 2$.

IV.—Equations of Class II.

The solution in this case will be of a different type, according as n is odd or even. As the first is far simpler, we shall take it first. We shall also defer till afterwards the general solution, and shall begin by finding a solution which will be infinite for the value zero of the variables $x_1, x_2, \ldots x_n$, and for all values of t. A solution of this equation is

$$f(u)\left[x_1^2 + x_2^2 + \ldots + x_n^2 - (t-u)^2\right]^{-\frac{1}{2}(n-1)},$$

where u is any constant. The sum of any number of similar terms is also a solution, provided that the path of summation is independent of $x_1, x_2, \ldots x_n$ or t.

Hence, putting

$$x_1^2 + x_2^2 + \ldots + x_n^2 = r^2,$$

we arrive at

$$\frac{1}{2\pi i}\int \frac{f(u)\,du}{[(u-t)^2 - r^2]^{\frac{1}{2}(n-1)}},$$

where u is complex, and the integration is taken about a closed contour in the plane of u. The result of the integration is, in general, zero, unless the contour encloses a pole of the subject of integration, such as

$$u = t - r, \quad \text{or} \quad u = t + r;$$

for $\frac{1}{2}(n-1)$ is integral, and $f(u)$ is taken to be a one-valued arbitrary function. Let us integrate along a contour enclosing the point $u = t - r$, and no pole of $f(u)$; then the result will be a finite series of negative powers of r, of which the most important term, when r is small, is

$$\frac{f(t-r)}{r^{\frac{1}{2}(n-1)}};$$

and when r is large,

$$\frac{f^{(n)}(t-r)}{r^{\frac{1}{2}(n-1)}},$$

where $f^{(n)}$ denotes the n^{th} derivative of f, it being supposed that this function and its derivatives do not become indefinitely large with r.

It will be noticed that when the functions f are exponential or circular, we obtain a result proportional to the Bessel functions of order, an integer $+\frac{1}{2}$. The solutions thus arrived at are infinite when $r = 0$, *i.e.* when

$$x_1 = 0, \quad x_2 = 0 \ \ldots \ x_n = 0,$$

without reference to the value of t; in fact, we get a "line" singularity, not a "point" singularity, in this space of $(n+1)$ dimensions. Another way of getting these results will be useful afterwards. We require a solution involving only r and t. The differential equation may then be written

$$\frac{\partial^2 \phi}{\partial r^2} + \frac{n-1}{r}\frac{\partial \phi}{\partial r} = \frac{\partial^2 \phi}{\partial t^2}.$$

On substituting for ϕ, $r^{-\frac{1}{2}(n-1)}F$, we get

$$\frac{\partial^2 F}{\partial r^2} - \frac{\partial^2 F}{\partial t^2} - \frac{(n-1)(n-3)}{4r^2}F = 0.$$

Let us assume for F the form

$$\psi + A_1\psi_1 + A_2\psi_2 + \ldots$$

when ψ, ψ_1 . . . are functions of $r - t$, such that

$$\psi'_1 = \psi, \quad \psi'_2 = \psi_1, \quad \psi'_3 = \psi_2 \ldots,$$

the accents denoting differentiations, and A_1, A_2 . . . are functions of r. The result will be, on writing

$$\kappa^2 = \tfrac{1}{4}(n-1)(n-3),$$

$$\psi\left(2A'_1 - \frac{\kappa^2}{r^2}\right) + \psi_1\left(2A'_2 + A''_1 - \frac{\kappa^2}{r^2}A_1\right) + \psi_2\left(2A'_3 + A''_2 - \frac{\kappa^2}{r^2}A_2\right) + \ldots$$

Equating to zero the coefficients of the functions ψ, we get

$$A_1 = -\frac{\kappa^2}{2r} = -\frac{1}{8r}[(n-2)^2 - 1],$$

$$A_2 = \frac{\kappa^4 - 2\kappa^2}{8r^2} = \frac{1}{128r^2}[(n-2)^2 - 1][(n-2)^2 - 3^2], \quad \&c.$$

Hence, we get a series for ϕ in descending powers of r. It terminates if n be odd, but if n be even, the series does not terminate; but, by taking any member to terms, we get an "asymptotic" solution. When n is even, as before, a solution is

$$\int \frac{f(u)\,du}{[(u-t)^2 - r^2]^{\frac{1}{2}(n-1)}}.$$

The path of integration which we take now will be a line coming from $-\infty$ passing around the point $u = t - r$ and returning to $-\infty$, or if we regard the u-plane as being a two-sheeted Riemann surface connected along the line joining $t - r$ to $t + r$, a line passing from $-\infty$ in one sheet to $-\infty$ in the other. We shall call this integral

$$K_{\frac{1}{2}(n-2)}(t, r),$$

and we shall speak of the function f as its generating functior

We easily find

$$K_{\frac{1}{2}(n-2)} = \frac{1}{1.3.5 \ldots (n-3)}\left(\frac{\partial}{r\partial r}\right)^{\frac{1}{2}(n-2)} K_0.$$

Hence, we may begin by discussing the function K_0, or

$$\int \frac{f(u)\,du}{[(u-t)^2 - r^2]^{\frac{1}{2}}}.$$

The path of integration may be deformed into (1) the real axis from $-\infty$ to the point $t-r-\epsilon$, where ϵ is a small positive quantity; (2) a small circle of radius ϵ surrounding the point $t-r$; (3) the real axis from $t-r-\epsilon$ to $-\infty$. The integral (2) vanishes with ϵ, and in (3) the subject of integration changes sign on account of the passage around the branch point. Hence

$$K_0 = 2\int_{-\infty}^{t-r} \frac{f(u)\,du}{[(u-t)^2 - r^2]^{\frac{1}{2}}} = 2\int_1^\infty \frac{f(t-rv)\,dv}{\sqrt{v^2-1}}$$

$$= 2\int_1^\infty \frac{f(t-rv)\,dv}{v}\left[1 + \tfrac{1}{2}v^{-2} + \frac{1\cdot 3}{2\cdot 4}v^{-4} + \ldots\right],$$

since $v > 1$.

Let us suppose $f(t-rv)$ does not become infinite for any real value of v from 0 to ∞, and vanishes when $v = \infty$; and that it can be expanded by Taylor's theorem in positive integral powers of rv, provided $rv < S$, where S is some positive quantity. Then

$$\int_1^\infty \frac{f(t-rv)\,dv}{v} = \int_0^\infty \frac{f(t-rv) - f(t-v)\,dv}{v} - \int_0^1 \frac{f(t-rv) - f(t)\,dv}{v}$$

$$- \int_0^1 \frac{f(t) - f(t-v) - f\left(t - \frac{1}{v}\right)dv}{v}.$$

The first integral is $-f(t)\log r$, and the second is a power-series in r, and the third integral is a finite constant; for the subject of integration is always finite, and so is the range of integration. For example, when $f(t)$ is e^t, it becomes $-\gamma e^t$, where γ is Euler's constant. Hence

$$\int_1^\infty \frac{f(t-rv)\,dv}{v} = -f(t)\log r + \alpha,$$

where α denotes a power-series in r.

In a similar manner

$$\int_1^\infty \frac{f(t-rv)\,dv}{v^{p+1}} = -\frac{f^{(p)}(t)r^p \log r}{p!} + \alpha_p.$$

Hence

$$2\int_1^\infty \frac{f(t-rv)\,dv}{\sqrt{v^2-1}} = -\log r^2\left[f(t) + \frac{r^2 f''(t)}{2^2} + \frac{r^4 f^{(\mathrm{iv})}(t)}{2^2 \cdot 4^2} + \ldots\right] + \beta,$$

where β is a power-series.

Hence, in the function $K_{\frac{1}{2}(n-2)}$, the term of most importance at the origin will be $f(t)r^{-(n-2)}$ multiplied by a numerical constant. We can get the asymptotic expression as follow:—Putting $u = t - r - w$, we get

$$K_{\frac{1}{2}(n-2)} = \int \frac{f[t-r-w]\,dw}{[2wr + w^2]^{\frac{1}{2}(n-1)}};$$

the path of integration goes from $+\infty$ around the origin and back again. We can divide it into two parts—(1) from $+\infty$ to $2r$ along the axis (this is traversed twice), (2) a closed path surrounding the origin and passing through the point $w = 2r$. The first will become of less importance, as $2r$ becomes great, and its limit is zero when multiplied by $r^{\frac{1}{2}(n-1)}$, and r made infinite. In the second we can expand

$$[2wr + w^2]^{-\frac{1}{2}(n-1)}$$

in a series of inverse powers of w. The principal term is

$$\frac{1}{r^{\frac{1}{2}(n-1)}}\int^{(2r)} \frac{f(t-r-w)\,dw}{[2w]^{\frac{1}{2}(n-1)}},$$

which, when r becomes very great, is

$$\frac{1}{r^{\frac{1}{2}(n-1)}}\int^{(\infty)} \frac{f[t-r-w]\,dw}{[2w]^{\frac{1}{2}(n-1)}} = \frac{1}{r^{\frac{1}{2}(n-1)}}\psi(t-r), \text{ say.}$$

This, then, is the first term of the asymptotic expansion, for it satisfies the definition of such an expansion, *i.e.*,

$$\underset{r=\infty}{Lt}\, r^{\frac{1}{2}(n-1)}\left[K_{\frac{1}{2}(n-2)} - \frac{\psi(t-r)}{r^{\frac{1}{2}(n-1)}}\right] = 0.$$

Further terms of the expansion can be found by means of the differential equation. It may be remarked that the phase of any disturbance in a space of odd dimensions depends on $t - r$, but that, in a space of even dimensions, the phase of a periodic disturbance will differ by an odd multiple of

$$\frac{\pi}{4}.$$

V.—EQUATIONS OF CLASS III.

We have now to consider the equation

$$\frac{\partial^2\phi}{\partial x_1^2} + \frac{\partial^2\phi}{\partial x_2^2} + \ldots + \frac{\partial^2\phi}{\partial x_n^2} - \frac{\partial^2\phi}{\partial y_1^2} - \frac{\partial^2\phi}{\partial y_2^2} - \ldots - \frac{\partial^2\phi}{\partial y_m^2} = 0.$$

Let α denote any solution of

$$\frac{\partial^2\phi}{\partial x_1^2} + \ldots + \frac{\partial^2\phi}{\partial x_n^2} = \frac{\partial^2\phi}{\partial u^2},$$

and β denote any solution of

$$\frac{\partial^2\phi}{\partial y_1^2} + \ldots + \frac{\partial^2\phi}{\partial y_m^2} = \frac{\partial^2\phi}{\partial v^2},$$

and f a function of u and v; then the result of substituting $\alpha\beta f$ for ϕ in the equation will be

$$f\beta\,\frac{\partial^2\alpha}{\partial u^2} - f\alpha\,\frac{\partial^2\beta}{\partial v^2},$$

or

$$\frac{\partial}{\partial u}\left(f\beta\,\frac{\partial\alpha}{\partial u} - \alpha\beta\,\frac{\partial f}{\partial u}\right) - \frac{\partial}{\partial v}\left(f\alpha\,\frac{\partial\beta}{dv} - \alpha\beta\frac{\partial f}{\partial v}\right) + \alpha\beta\left(\frac{\partial^2 f}{\partial u^2} - \frac{\partial^2 f}{\partial v^2}\right).$$

Hence, if we take f to be a function of $u \pm v$, and α and β to be singular solutions of the respective equations, then the solution will be

$$\iint dudv\alpha\beta f(u \pm v).$$

If m and n are odd, and if we put

$$r^2 = x_1^2 + x_2^2 + \ldots + x_n^2,\ \rho^2 = y_1^2 + y_2^2 + \ldots + y_n^2.$$

This solution will consist of a finite series of terms of the form

$$r^{-p}\rho^{-q} f(r \pm \rho).$$

We write this solution

$$\int\int du\,dv\,f(u \pm v)\left[u^2 - r^2\right]^{-\frac{1}{2}(n-1)}\left[v^2 - \rho^2\right]^{-\frac{1}{2}(m-1)} = K_{\frac{1}{2}(n-2),\,\frac{1}{2}(m-2)}(r,\,\rho);$$

and it can obviously be expressed in various ways as a sum of functions

$$K_{\frac{1}{2}(n-2)}, \quad \text{or of} \quad K_{\frac{1}{2}(m-2)}.$$

VI.—Non-Homogeneous Equations.

By a linear substitution, the equation can be written in the form

$$\frac{\partial^2\phi}{\partial x_1^2} \pm \frac{\partial^2\phi}{\partial x_2^2} + \ldots + a\frac{\partial^2\phi}{\partial y^2} + b\frac{\partial\phi}{\partial y} + c\phi = 0.$$

If a does not vanish, by the substitution

$$\phi = e^{-\frac{b}{2a}y}\,\psi,$$

we get

$$\frac{\partial^2\psi}{\partial x_1^2} \pm \frac{\partial^2\psi}{\partial x_2^2} + \ldots + a\frac{\partial^2\psi}{\partial y^2} + c'\psi = 0.$$

If $c' = 0$, *i.e.* if $b^2 = 4ac$, the equation is homogeneous. If $c' \neq 0$, the equation to be solved is of the form

$$\frac{\partial^2\psi}{\partial x_1^2} + \frac{\partial^2\psi}{\partial x_2^2} + \ldots + \frac{\partial^2\psi}{\partial x_n^2} - \psi = \frac{\partial^2\psi}{\partial y_1^2} + \ldots + \frac{\partial^2\psi}{\partial y_m^2}.$$

If $\chi = e^{\tau}\psi$, where ψ does not contain τ, and τ is a new variable, then we have got to get a solution χ of

$$\frac{\partial^2\chi}{\partial x_1^2} + \ldots + \frac{\partial^2\chi}{\partial x_n^2} - \frac{\partial^2\chi}{\partial\tau^2} = \frac{\partial^2\chi}{\partial y_1^2} + \ldots + \frac{\partial^2\chi}{\partial y_m^2},$$

such that $e^{-\tau}\chi$ will not contain τ.

Such a solution is

$$\rho^{-\frac{1}{2}(m-2)}\int\frac{e^{-u}\,du}{\left[(u-\tau)^2 - \tau^2\right]^{\frac{1}{2}(n-1)}} = \rho^{-\frac{1}{2}(m-2)}\int\frac{e^{\tau-v}\,dv}{(v^2 - r^2)^{\frac{1}{2}(n-1)}},$$

and hence

$$\psi = \rho^{-\frac{1}{2}(m-2)}\int\frac{e^{-v}\,dv}{(v^2 - r^2)^{\frac{1}{2}(n-1)}}.$$

If a vanishes, the type of the equation is

$$\frac{\partial^2\phi}{\partial x_1^2}+\frac{\partial^2\phi}{\partial x_2^2}+\ldots+\frac{\partial^2\phi}{\partial x_n^2}-\frac{\partial\phi}{\partial y}=\frac{\partial^2\phi}{\partial y_1^2}+\ldots+\frac{\partial^2\phi}{\partial y_m^2}.$$

In a similar manner, we get

$$\phi=\rho^{-\frac{1}{2}(m-2)}\int\frac{e^{-v}dv}{(r^2-vy)^{\frac{1}{2}(n-1)}}.$$

If n is even, we take the integral surrounding ∞ and $\frac{r^2}{y}$; and if n be odd, we take a contour about $\frac{r^2}{y}$. Hence,

$$\phi=\text{const. }\rho^{-\frac{1}{2}(m-2)}y^{-\frac{1}{2}(n-1)}e^{-\frac{r^2}{y}}.$$

From this we can get different solutions, as follows:—

$$\phi=\rho^{-\frac{1}{2}(m-2)}e^{-py}\int(y-v)^{-\frac{1}{2}(n-1)}e^{-\frac{r^2}{y-v}+p(y-v)}\,dv.$$

The integral is, of course, a Bessel function.

VII.—NON-SYMMETRICAL SOLUTIONS.

All the above solutions belong to the equations

$$\frac{\partial^2\phi}{\partial r^2}+\frac{n-1}{r}\frac{\partial\phi}{\partial r}=0,$$

$$\frac{\partial^2\phi}{\partial r^2}+\frac{n-1}{r}\frac{\partial\phi}{\partial r}=\frac{\partial^2\phi}{\partial t^2},$$

$$\frac{\partial^2\phi}{\partial r^2}+\frac{n-1}{r}\frac{\partial\phi}{\partial r}=\frac{\partial^2\phi}{\partial\rho^2}+\frac{m-1}{\rho}\frac{\partial\phi}{\partial\rho}.$$

We can get non-symmetrical solutions as follows:—For example, in the last equation: let S_p, Σ_q be homogeneous functions of integral degrees p and q, respectively, in $x_1, x_2, \ldots x_n$; $y_1, y_2, \ldots y_n$, and let them be solutions respectively of

$$\frac{\partial^2\phi}{\partial x_1^2}+\ldots+\frac{\partial^2\phi}{\partial x_n^2}=0,$$

and of

$$\frac{\partial^2\phi}{\partial y_1^2}+\ldots+\frac{\partial^2\phi}{\partial y_m^2}=0.$$

Then if $S_p \Sigma_q \psi$ is a solution where ψ is a function of r and ρ, then

$$\frac{\partial^2 \psi}{\partial r^2} + \frac{n+2p-1}{r} \frac{\partial \psi}{\partial r} = \frac{\partial^2 \psi}{\partial \rho^2} + \frac{n+2q-1}{\rho} \frac{\partial \psi}{\partial \rho},$$

so that

$$\psi = K_{p+\frac{1}{2}(n-2),\ q+\frac{1}{2}(m-2)}(r, \rho).$$

In like manner, a general solution of the second equation is

$$S_p K_{p+\frac{1}{2}(n-2)}(r, t).$$

VIII.—Kinetic Solutions.

All the above results admit of being extended in an important manner. For brevity, our remarks will be confined to solutions of Class II., in which the total number of variables is even. Writing

$$r^2 = (x_1 - x'_1)^2 + (x_2 - x'_2)^2 + \ldots,$$

the integrals are of the form

$$\int du f(u) [r^2 - (t-u)^2]^m,$$

where m is an integer. This function is infinite for the values

$$x_1 = x'_1; \quad x_2 = x'_2, \quad \&c.,$$

and for an infinite range of values of t. To extend this result, let $x'_1, x'_2 \ldots$ be functions of u. Then the integral will be taken about a real zero of the function

$$r^2 - (t-u)^2;$$

and an infinity will occur when, for a zero u of this function,

$$\frac{\partial}{\partial u}[r^2 - (t-u)^2] = 0, \quad \text{or} \quad r\frac{dr}{du} + t - u = 0.$$

Hence, at an infinity we must have

$$r^2 = \left(r\frac{dr}{du}\right)^2.$$

But

$$r\frac{dr}{du} = -(x_1 - x'_1)\frac{\partial x'_1}{du} - \&c.;$$

and, since the numbers are real, the square of this expression will be less than r^2, if

$$\left(\frac{\partial x'_1}{\partial u}\right)^2 + \left(\frac{\partial x'_2}{\partial u}\right)^2 + \ldots < 1.$$

Hence, with this condition, we must have

$$r = 0, \quad i.e. \quad x_1 = x'_1; \quad x_2 = x'_2, \quad \&c.;$$

and hence $u = t$. Hence the solution is infinite for all the values of the variables given by the equation

$$x_1 = x'_1(t); \quad x_2 = x'_2(t), \quad \&c.,$$

where $x'_1(t)\ldots$ denotes the result of writing t for u in $x'_1\ldots$ To this type of solution we may give the name of Kinetic Function. For, if t denotes the time, such a solution would have a singularity which was in motion.

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